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NOTES AND COMMENTS.

The Little Trip of the "Fram."

WELL! we *have* made a mistake. "Skâl to the Norseman!" we cried in the innocent enthusiasm of our leading article, after reading Nansen's first telegram to the *Daily Chronicle*, for we thought he had achieved a magnificent success. But a paper by Mr. A. M. Brice in the December number of the Geographical Society's *Journal* shows up our error: "the drift of the Fram—full of marvel though it be—has only extended our knowledge a few degrees in one direction."

When this paper was read, the President of the Geographical Society assured the meeting of the brilliant ability of its author. Its estimate of Nansen's results appears therefore with some authority, and we turn eagerly to see what were the achievements wrought in Franz Josef Land, beside which those of Nansen pale. The map illustrating the paper represents two years' work in Franz Josef Land. It is nothing that we are disappointed when comparing it with the work done by Payer in one month's march, or with the survey made by Leigh Smith in two short cruises along the south coast; for if Mr. Harmsworth, who has so generously paid for the work, does not grumble at the time, no one else need object—except when previous explorers are unfairly treated.

What is the great result of this two years? Mr. Jackson left his village at Cape Flora "to discover if possible, new land to the westward." This boat-cruise resulted in the "discovery" of a majestic headland, which "newly found cape received the name of Cape Mary Harmsworth," a discovery which, it is claimed, "has advanced Franz Josef Land a considerable distance westward of the previously known limit," and rediscovered Gilies Land. Interested in this great discovery, we turn to the map only to recognise in the "newly found cape" our old friend Cape Lofley, and to see that the southern coast of Franz Josef Land has been extended westward no further than it was already known from the work of Leigh Smith. That courageous but modest explorer was unable to take his yacht the "Eira" further west than the entrance to the fjord, which he named Cambridge Bay.

He saw to the west two capes forming the south-eastern and apparently the south-western extremities of the island called Alexandra Land: the former he named Cape Ludlow, the latter, Cape Lofley. Cape Lofley, though seen only in the distance, stood up distinctly as a bold rocky headland, and was marked on Leigh Smith's chart as far on one side of Cape Ludlow as Cape Crowther is on the other, a position exactly agreeing with that assigned in this new map to Cape Mary Harmsworth. Not that the name Cape Lofley has disappeared: Jackson has removed it to an insignificant point between Leigh Smith's Cape Lofley and Cape Ludlow, a point which Leigh Smith, considering the distance which the "Eira" was from it, could not have distinguished from the land behind it.

Mr. Jackson's detailed map of the southern coast of Alexandra Land will doubtless be of great value when this part of the world is divided into building lots; but the claim that his exploration has advanced Franz Josef Land "a considerable distance to the westward of the previously known limits" is not supported by his map. The identification of Alexandra Land with Gilies Land is probably correct, but is by no means a new idea. Markham's "Threshold of the Unknown Regions" includes Gilies Land as part of Franz Josef Land. Reference to Sir Clement Markham's admirable work reminds us of another instance of the Jackson-Harmsworthian method of treating the names of their predecessors. Payer named the great sound which extends along the south-eastern shore of Zichy Land, and thence to Cape Fiume, "Markham Sound." Leigh Smith accepted the name and mapped the south-western end of the sound. Now on the present map this great sound is re-christened the "British Channel," while "Markham Sound" is restricted to an unimportant passage between the island on which stand Payer's Capes Fiume and Triest, and the three opposite islands. We regret a proposal to remove the name "Markham" from the channel that is likely to be the main route followed in future exploration of the area north of Franz Josef Land.

Perhaps the most valuable item of information given by Mr. Jackson's map is the position of Nansen's winter quarters; for it enables us to understand that explorer's criticism of Payer's map. According to one of Jackson's letters "Payer's map being so utterly wrong, had completely fogged Nansen; and at his winter hut, he believed himself near Cape Lofley, and that the land to the westward, which we had discovered (!) was Spitzbergen." Nansen, therefore, according to Mr. Jackson was very considerably fogged, for he was about 13° out in longitude and three-quarters of a degree in latitude. No wonder then that he could not reconcile his observations with Payer's map, and the fact that he did not see Petermann Land where he expected it, is no proof of its non-existence.

Nansen, however, may pluck up heart of grace. His journey will yet live in history, thanks to the magnanimity of his rescuers.

Finding that "Nansen has very little of his own to name," and that he was apparently disappointed at being "unable to name anything in the neighbourhood where he passed a winter under such conditions," Mr. Jackson offered to allow Nansen to name the island. This offer Mr. Jackson's expositor describes as an act of "single-mindedness, generosity, and an entire absence of anything approaching a petty sense of rivalry . . . difficult to beat in the annals of exploration." Jackson remarked that the naming of this country "only rests between himself, [Nansen] and me." The fact that it had been previously named the Karl Alexander Land by Payer, as the new map in the *Geographical Journal* shows, is a trivial detail that Mr. Jackson seems to have overlooked. Nansen, however, appears to have understood this; he is a man of humour, and entered into the spirit of the joke. So he promptly called the country "Frederick Jackson Island," apparently as a mark of his appreciation of the originality of the discoveries of his rescuer, whose gracious permission to re-name an already named island has conferred immortality upon the little trip of the "Fram."

The Value of Morphology.

The suggestive paper by Mr. MacBride, appearing in our present number, states many of the problems now engaging the earnest attention of biologists, botanists no less than zoologists. The great question before us really is: What do we mean by Biology; what is the aim of our research? Put broadly, the object of those who think on such matters is no doubt to fit all the phenomena of life, both physical and psychical, into some general scheme of the philosophy of the universe, alongside the phenomena of inorganic matter. If once this could be accomplished, all questions of minor philosophic, but major practical, interest would receive their answers by simple deduction. Till then, of course, our energies will constantly be distracted towards the solution of problems bearing on the daily life of our own species. But setting bread-and-butter considerations aside for the moment, and placing before us only the grand enigma, we have each of us to test our own line of investigation by its concordance with this main direction of philosophic enquiry.

Mr. MacBride discusses a few of the methods of zoology, but chiefly the morphological method. How far, he says in effect, does the comparison of the structure of the various species and genera of animals shed light on the relation of life to not-life, on the steps and the causes of evolution, on the problems of heredity, and on other obscurities that need illumination? This line of attack has been much criticised of late: there are earnest workers, to whom shallowness of thought cannot be imputed, who reject the methods of dissection, description, minute comparison, and systematic analysis of species. They say that these methods will not stand the touch-stone; that they are barren of philosophic fruit. Mr. MacBride does not

agree, or, at all events, he is not ready to swap horses yet. While realising the value of Statistical Zoology, of Experimental Embryology, and other new methods, we, too, are not inclined to discard either morphological or systematic investigation. Indeed, we consider that morphology is only just beginning to see the size and shape of the field that it has to till. But we trust that this paper may open a discussion of much interest and value.

"A PLAGUE O' BOTH YOUR HOUSES!"

AFTER all, what do we know of the plan of the universe? We do not even know that there is a plan. Our need still is for facts. And the immediate value of any method or any theory is its incitement to collect and to discover fresh facts. Within the limits of biology we seem to see many hypotheses at present incapable of proof, but many methods of research all capable of advancing knowledge. We would suggest that the present is not the time for speculation on the merits or failings of Natural Selection or any other theory as to origin of species. A truce to library papers for ten or twenty years! Meanwhile let those naturalists for whom the subject has any fascination forget their pet theories, and set to work. They may find a pattern in a familiar series of green-bound books published by Murray. Perhaps at the end of the suggested period it may be possible to formulate important results based on experimental evidence, say, for instance, whether variation is indefinite or definite; to understand something of correlated variation and of the extent to which it must be considered as a factor in the problem, and so on, and so on. Perhaps we may seem no nearer the solution of these and similar questions. But a great deal of useful work will have been done, our stock of knowledge will have been increased, and we shall not have to lament the waste of valuable time on empty discussion. We are urged to these remarks by a paper read at a recent meeting of the Linnean Society, entitled "Does Natural Selection play any part in the Origin of Species among plants?" In the opinion of its reader it did not, the key of the whole problem being the reaction of plant and environment. This view is, of course, not a new one. Nor were any new experiments advanced in support of it. The chief evidence put forward lay in the production of several of our useful vegetables, like the parsnip and carrot, from a wild plant by cultivation. The same and similar facts, in the opinion of many who, following Darwin, find in Natural Selection a conceivable hypothesis, point in the opposite direction. In view of such marked disagreement it is manifest that existing evidence is insufficient. There is nothing for it but to continue the patient plodding investigations initiated by the author of the "Origin of Species" and (let us not forget) of all those other works choke-full of experiment, ingeniously planned and carefully carried out.

Alternation of Generations in Vertebrates.

We have had some difficulty in finding an appropriate heading for this comment on a recent treatise by Mr. John Beard, entitled "On Certain Problems of Vertebrate Embryology," and published at Jena by Fischer. It deals incidentally with speculations as to the ancestry of vertebrates, such as Dr. Gaskell's theory of arthropod origin alluded to in our November number; it discusses many particular problems of embryology, such as the fate of the yolk and Mr. Beard's own discovery of an ephemeral nervous system in vertebrate larvæ; it has also an important bearing upon what is called the recapitulation theory. But probably the central and most important idea in it is the suggestion that all Metazoa, and in particular the Vertebrata, have representatives of the two generations well known in plants, the sporophyte or asexual generation, and the gametophyte or sexual generation. Mr. Beard's position among vertebrate embryologists is so assured, and this particular volume is so important a summing-up of his work for the last eight years, that specialists certainly will study the treatise for themselves. We shall attempt, therefore, in an untechnical fashion to set forth the general lines of his brilliant theory.

Two types of larvæ are familiar to the naturalist, although in many instances there is difficulty in determining under which type particular larvæ must be grouped. In one of these types the larva, although it may attain a large size and be provided with special organs, is no more than a bearer of the growing adult. The real embryo appears as a small bud in some part of the large bearer, and gradually grows until a point is reached at which it has become more important than its nurse. The nurse or bearer then degenerates, and frequently its broken down tissues are absorbed and digested by the growing embryo. Such a larva, familiar among the echinoderms, may be compared with the sporogonium of a moss, and, indeed, Mr. Beard declares it to be, like the sporogonium, an alternating, sexless generation. On the other hand, there are larvæ, like tadpoles, and probably like those of at least most insects, which pass directly into the adult form, changes no doubt taking place, but yet the general structure of the larva being directly transformed into the general structure of the adult. Such a larva may be compared to a young seedling, the leaves and shape of which may differ from the adult but which directly becomes the adult. Mr. Beard's great thesis is that in all vertebrate development there is a stage easy to make out and directly comparable to the first kind of larva, that is to say, to an asexual, alternating generation. At a definite point of the development, different in date from the beginning of the development in different vertebrates but corresponding very closely in the differentiation of organs attained, what he calls the critical stage is reached. The embryo has become a fœtus; the main lines of the future organism have been laid down, and a sudden change in the mode of nutrition marks the degeneration of those structures which correspond to the asexual

generation. Thus in a dogfish at the critical stage the embryo is definitely a dogfish, with signs of the peculiar snout, paired and unpaired fins. Up to this stage the nutrition has been through the medium of special cells of the external yolk-sack which have emulsified, and, so to speak, made pap of the yolk. After this stage, the yolk is gradually drawn up into a new and internal yolk-sack, formed from tissue which becomes an actual part of the future fish; and the yolk is digested directly by the alimentary canal of the future fish, the pancreas of which has begun to secrete. The external yolk-sack disappears. A corresponding stage Mr. Beard thinks to have made out, or at the least to have pointed to, in all the vertebrate groups. In the case of opossums and marsupials without the allantoic placenta described by Mr. Hill, the embryo is born at the critical stage, and, as the yolk-sack of these and higher animals is already devoid of yolk, the change in nutrition is not from external to internal yolk-feeding, but to the new diet of milk. Mr. Beard considers that the milk-glands are older than the true placenta, and that their coming into play marks the change in the nature of the embryo. In animals with an allantoic placenta the critical stage is marked, not by birth, but by change from yolk-sack placenta, such as is found in the marsupials, to the allantoic placenta, which is an outgrowth from the true embryo, not from the yolk-sack. As the allantoic placenta became more important, the sphere of the mammæ became less important, and indeed Mr. Beard looks to the ultimate disappearance of suckling. Thus, in some ungulates the young are nourished so long in the uterus that a few hours after birth they are able to feed on grass. As having a possible bearing in favour of his view that nutrition by milk is older than nutrition by a true placenta, we may remind him that one of the earliest signs of pregnancy in woman is change and activity in the milk-glands.

At the critical stage in all vertebrate embryos, a similar degree of development has been attained: the central canal of the nervous system has begun to form, constriction of the notochord has begun, the sex has been determined, and the permanent kidney has been formed. But where Mr. Beard charges against the theory of recapitulation is this: at the critical stage the final characters of the adult are sufficiently marked to determine the young embryos as respectively fish, amphibian, reptile, bird, or mammal. Indeed, Mr. Beard goes further, and declares that in the case of the human embryo the critical stage is definitely human. To put the difficulty plainly: in its crudest form the recapitulation theory supposes that the mammalian embryo passes through a fish-like stage when the gill-slits are developed. Mr. Beard declares that before the gill-slits have appeared the embryo has already assumed definite mammalian characters: that it becomes a mammal before it becomes a fish.

It would be unfair and unwise to criticise Mr. Beard's conclusions when we have found too little space for his arguments; but

it is worth pointing out that when Mr. Beard carries so far back in ontogeny the meeting place of the different vertebrate groups, he is only taking an unusually long step in the direction in which zoologists generally have been proceeding. At first, the attempt was made to arrange vertebrate groups in linear form, so that on the recapitulation theory any high mammal would pass through a number of stages corresponding, as one traced it backwards, first to lower mammals, then to a series of reptiles, then to a series of fish, and so on. Now zoologists have been busy at work arranging the groups in diverging lines from remote common ancestors. In fact, we should not be surprised to see it argued on the grounds of comparative anatomy that mammals, Sauropsida, and Ichthyopsida had come independently from a common stock.

Although the law of recapitulation was drawn from considerations of mammalian embryology, upon which Mr. Beard places an interpretation opposing it, we are prepared to support the apparent paradox that a mammal may have been a mammal before it was a fish, and may yet recapitulate in its ontogeny some of its ancestral history.

Wild Hybrids between the Arctic and Common Foxes.

One of the causes which is most frequently invoked by various persons to account for the presence of unexpected characters in animals is crossing, and yet, as a matter of fact, hybridisation occurring in the wild state and not in a menagerie, is one of the very rarest phenomena possible, and, within the group of Mammalia, the undoubted instances of it known might almost be counted on the fingers.

This being the case, we need make no apology for drawing the attention of naturalists to Dr. Einar Lönnberg's interesting account, published in a Swedish sporting journal, of an undoubted case of hybridisation in the wild state between those very different species, the Arctic and Common Foxes. ("En Bastard mellan fjellräf och vanlig räf," in *Svenska Jägarförbundets Nya Tidskrift*, xxxiv. pp. 154–164, with coloured plate, 1896.)

In this paper Dr. Lönnberg carefully describes two hybrid specimens between the above species, now in the Upsala Museum. He conclusively shows their intermediate characters, and has happily been also able to make out something of their history. For in a private letter he tells us that no less than nine hybrid foxes were shot or trapped within a period of two years, and these are clearly shown to have been the offspring of a female Arctic fox, which had mated with the common foxes of the district of Öregrund. He furthermore tells us, which is highly important, that the place where the hybrids were bred is far south of the usual limit of the Arctic fox, and that no other specimen but the presumed parent had ever been seen there.

It is, no doubt, very difficult to obtain direct and conclusive evidence of such cases of wild hybridisation as may occur, but it may be remarked that of thousands of specimens that have passed under the writer's ken during the last twenty years, only one single individual could be at all confidently presumed to be a hybrid, and in that case, just as in this, the occurrence took place in a district where, owing to much shooting down of game, one or both parents might have found much difficulty in finding a mate of their own species.

Where this difficulty does not exist, owing to the abundance of both forms, there seems, as yet, no evidence that crossing ever takes place among mammals in a state of Nature, whatever may be the case with birds. But the very absence of such evidence makes it the more important that every apparent case should be properly investigated, and we are therefore very grateful to have Dr. Lönnberg's careful account of the Bastard Swedish Foxes.

The paper is illustrated by an excellent figure of one of the hybrids, showing very clearly how intermediate in colour it is between the two parent species.

THE BIOLOGY OF THE GREAT LAKES.

IN connection with Mr. Scourfield's plea for a British fresh-water biological station, appearing in this number, it is very instructive to note the increasing attention paid to the study of fresh-water animals and plants in the United States. As a good instance of this may be taken the work of the Michigan Fish Commission. This Commission not only performs the practical duties of fish-hatching and "planting," but has also carried out a considerable amount of valuable biological work, as may be seen from the six *Bulletins* it has already issued. The ultimate object in view has naturally been a practical one, namely, to ascertain the cause of the decreasing yield of whitefish (*Coregonus clupeiformis*) in spite of artificial propagation, evidently a subject of some importance to the State of Michigan, touching as it does four of the five Great Lakes. Recognising that the only basis for intelligent action in the future would be a more complete knowledge of all the conditions surrounding the whitefish, the Commission very wisely began a biological examination of the Great Lakes. During the summer of 1893 a party of naturalists, under the direction of Professor J. E. Reighard, was maintained on Lake St. Clair, a great spawning-ground of the whitefish, lying between Lake Huron and Lake Erie, and in the following year a similar party, under Professor H. B. Ward, was stationed at Charlevoix on Lake Michigan. In both instances every effort was made to get as complete a picture as possible of the whole aquatic fauna and flora. Most of the groups of any importance were worked out by specialists, whose reports, published either as separate *Bulletins* or as appendices to the two principal reports (*Bulletins* Nos. 4 and 6), contain a mass of information, of use not only

to American workers but to biologists everywhere. The two reports on Rotatoria by Mr. H. S. Jennings, that on Cyclopidæ and Calanidæ by Professor C. D. Marsh, that on the Turbellaria by Dr. W. M. Woodworth, and that on the Parasitic Worms by Professor Ward, are especially noteworthy. But even more important than this work on the different groups of animals and plants was the application of the methods, recently developed by Hensen and Apstein, to the estimation of the amount and distribution of the "plankton," *i.e.*, of the minute plants and animals found floating free in the water, which form, in the last resort, almost the only source of the food-supply of fishes in large and deep lakes, and play a not unimportant part in this respect in smaller and shallower ones. The results obtained in this way are of the greatest scientific interest, confirming, as they do, many of the observations of Apstein, Zacharias, and others on the plankton of European lakes; nor can they fail to be of value in the practical work of fish-culture. The Commission is certainly deserving of all praise for undertaking this biological examination of the Great Lakes, and we can only regret that the work cannot be carried on continuously, instead of being limited to a few weeks in the summer.

In connection with this subject, we may mention that we have received a copy of "The Illini," the weekly journal of the University of Illinois, giving an account of its biological experiment station, the first to be started in the United States with an adequate equipment. This station also differs from others in having for its basis of work the system of life in a river. The article, which is written by Mr. C. A. Kofoid, has some excellent illustrations, and we hope it will soon be possible to give an equally interesting account of a British fresh-water biological station.

A Starfish "Struggle-for-Lifer."

There lives in the Mediterranean, as well as in the adjacent parts of the Atlantic, an ally of our common crossfish, known as *Asterias richardi*. Professor Perrier, who first described the species, noticed the peculiar fact that the young were provided with six arms, whereas the adults had no more than five. Since he also observed cases in which three of the arms were much smaller than the rest, he assumed that the species was accustomed to reproduce by fission during its youth, but that in its old age it sobered down to the more usual method of reproduction. It appears, however, from the observations of Dr. E. von Marenzeller, that the arms are thrown off, after the not uncommon custom of echinoderms, while the disc that remains behind buds out fresh arms (*See* Natural Science, vol. v., p. 4, July, 1894.). The curious point remains to be explained, that the young is more given to this operation than is the adult, as well as the fact that in old age there seems to be a cessation of regeneration, so that some individuals have only two or three arms.

The reason, according to Dr. von Marenzeller, is as follows:—

There is a small parasitic animal, whose affinities can hardly as yet be considered fully determined, but which is well known as a parasite in and upon crinoids, and is called *Myzostoma*. A species of this enters the mouth of *Asterias richardi*, and, taking up its abode in the blind extensions of the stomach that pass into the arms, it there grows to a size and breadth that are quite remarkable for a *Myzostoma*. The *Asterias* soon feels inconvenienced, but, being unable to get rid of the intruder by the way through which it entered, it is compelled to adopt the rather severe measure of getting rid of the arm in which the *Myzostoma* has esconced itself. The more numerous the parasites, the greater the number of arms that have to be disposed of in this summary fashion. At first the starfish, endowed as all starfish are with strong powers of recuperation, re-grows all its six arms; but after a time it finds it less trouble to close up the gap, and to content itself with five arms. And since by these repeated struggles in its youth its powers have been reduced to a greater extent than is customary in this class, it often finds itself forced to hobble out the remainder of its days with the miserable allowance (for a starfish) of three arms. The same parasite is found to occur in *Stolasterias neglecta*, another starfish with the same distribution, and it appears to have like effects. This case of autotomy or self-division has, then, no connection with reproduction; it is a simple example of a continuous struggle for life.

The complete account of this will be found in Dr. von Marenzeller's report on the Echinoderma collected in the Mediterranean in 1893–4 (*Denkschr. Akad. Wien, Math.-Nat. Classe*, lxii., pp. 123–148, 1895).

An Eastern Question.

What has Japan got to do with the East Coast of America? More than once of late have we drawn attention to curious instances where the distribution of a genus was confined to these two localities (vol. viii., p. 10, the Chimæroid *Hariotta*, and p. 350, the Octopod, *Opisthoteuthis*); and now, on glancing at Dr. Sluiter's recently-issued Catalogue of the Starfishes in the Amsterdam Museum, we note that the well-known species *Echinaster spinosus*, hitherto known only from the east coast of America, has—if labels are to be trusted—been found on the shores of Japan.

A Revision of North American Slugs.

A paper recently published by Messrs. H. E. Pilsbry and E. G. Vanatta in the *Proceedings* of the Academy of Natural Sciences, Philadelphia (1896, pp. 339–350) marks a great advance in the study of North American slugs, and forms the first of a series which will prove invaluable to all interested in those animals. Writers on the slugs of America have hitherto been accustomed to distinguish them according to their external characters, an imperfect method of work

which it may be remembered we have found it necessary to censure in other branches of systematic zoology. Here for the first time the introduction of new species is accompanied by descriptions and figures of the internal parts as well as of the external form, in a manner similar to that employed by such English and Continental malacologists as Simroth, Lessona, Pollonera, Godwin-Austen, Collinge, and Scharff. The present instalment treats of *Ariolimax* and a new genus *Aphallarion*. Two species of the former genus are figured and described, and descriptions given of varieties and insufficiently known forms. The genus *Aphallarion* is constituted for a new species, *A. buttoni*, from California, which, although very like an *Ariolimax* both externally and internally, differs in the complete absence of a penis and retractor muscle, agreeing in this feature with the condition which obtains in *Arion* and *Prophysaon*.

The *Ariolimax* from Costa Rica in the British Museum, described as a sub-species by Cockerell, is treated by the present authors as probably a good species "on account of its locality (if correct), but a diagnosis is still wanting." Perhaps Mr. Cockerell might retort that it is only a "full diagnosis" that is wanting. That may be; but how many forms there are in our valuable national collection of which we have as yet nothing more than a preliminary description! how many varieties which may be good species, or species which may be only varieties! And these remarks do not apply to slugs alone.

All Smoke and no Fire.

The informal smoking evening of the Geological Society, to celebrate the displacement of gas by electric light, to which we alluded in our last number, was not quite such a success as had been hoped. The President was unable to attend, owing to an indisposition from which he has since recovered, and very few of the Fellows themselves were present. The President, however, had sent down some excellent photographic lantern slides illustrating points in the geology of Devonshire and Cornwall, and these were explained by Mr. W. W. Watts. Perhaps, as Fellows only were admitted to this meeting, we should not be in order in giving a lengthy account of the proceedings. Fortunately, it is unnecessary to dwell upon the magic lantern entertainment on the Holy Land which Prof. Hull might have borrowed from the S.P.C.K. to amuse the Fellows with; but we may allude to the successful attempt made by Mr. Teall to project actual rock-sections upon the screen. The exhibits, too, were neither numerous nor, with one or two exceptions, particularly exciting. Dr. Wheelton Hind exhibited the specimens of *Carbonicola* described in the monograph recently reviewed by us, also a series of goniatites intended to show the differences in the cephalopod fauna of the Carboniferous shales and limestones. Professor Grenville Cole sent up specimens illustrating his paper on the igneous intrusions of Port

Glasdruman, Co. Down, and Professor Blake showed a sample of a ferruginous oolite from Cutch, the grains of which being covered with iron pyrites, caused it to be called "golden oolite."

THE ANTIQUITY OF MAN IN BRITAIN.

THE honours of this geological *soirée* went to Mr. W. J. Lewis Abbott, some of whose exhibits were indeed remarkable. From the Ightham Fissure alone he has increased Prestwich's list of thirty-seven British cave and fissure vertebrates to about ninety, all of which were shown, and among them one of the most interesting was *Canis lagopus*, the arctic fox. From the Hastings kitchen midden he has secured a large assemblage of diminutive implements, supposed for the most part to be fish-hooks, and to have been used by a peaceful race that in many parts of Europe were settled on the seashore, often in proximity to more warlike tribes. Concerning the customs of this race much information has been accumulated, and we hope in a forthcoming number to publish a paper by Mr. Abbott with illustrations of the extraordinary relics that he has found. He also had some remarkable specimens of stone-working discovered on the supposed sites of ruined cities of India. Their strangeness consisted in the fact that the stone had been chipped into almost perfect cubes and globes, a feat which the modern imitators of the stone-workers, including Mr. Abbott himself, are quite unable to perform; many of these specimens, too, were delicately ornamented, presumably by the burning of an alkali into patterns incised upon them.

But the interest of all these specimens was completely cast into the shade by some rough-looking stones lying on the table. These were flints which certainly bore a striking resemblance to the work of man, which we believe the most critical expert would say probably were the work of man. and which had been obtained by Mr. Abbott's own hands, in the presence of a witness, from the Cromer Forest Bed at Runton, where they were found sticking in the iron pan, portions of which were still attached to them. One of them showed an undoubted bulb of percussion. We shall publish next month an illustrated account of these specimens, which are among the most interesting evidences of human antiquity that have been turned up for many a long year. The Forest Bed, we may remind those of our readers who are not geologists, lies, according to Prestwich, at the base of the Pleistocene or Quaternary system, but is now usually regarded as forming the top of the Pliocene series; it contains remains of the cave-bear, of the rhinoceros, of the hippopotamus, various species of elephant, deer, and other species of mammals, both living and extinct. In this country, at all events, no one has ever professed to find the remains of man at so low a horizon, although the opinion

has before now been hazarded that if they occurred at this horizon at all, they would be found at the place where Mr. Abbott has actually discovered them.

Giant Ammonites.

We understand that the British Museum (Natural History) has acquired a magnificent specimen of the ammonite *Haploceras leptophyllus*, Sharpe, from the Upper Chalk of Rottingdean. It was one of seven that were to be seen this summer between Brighton and Newhaven, the largest of which reached the gigantic size of 5ft. 6in. across, without the body chamber, but was almost destroyed when discovered, and could not be moved. The specimen now at the Museum is in a perfect condition, is 44 inches across, and shows a few inches only of the body chamber. We believe that Mr. Crick will contribute a note on its features and peculiarities to the *Geological Magazine*, a note we shall await with considerable interest.

Had the Belemnite an Aptychus?

Our recent remarks on the relations between stratigraphers and palæontologists find an appropriate illustration in a paper contributed by Dr. H. P. Blackmore, of Salisbury, to the *Geological Magazine* for December. There are found in the Chalk, as in earlier Mesozoic strata, many of those peculiar bodies known as aptychi, and now generally accepted as being the opercula of ammonites, corresponding to the hood of the recent *Nautilus* (see Natural Science, vol. viii., p. 84, Feb., 1896). There is no question as to the correctness of this view in the majority of cases; but Dr. Blackmore, carefully collecting zone by zone in his own neighbourhood, established the facts "that *Aptychus leptophyllus* and *Belemnitella lanceolata* always occurred together, and that *Aptychus Portlockii* and *Belemnitella quadrata* were similarly associated," and "that *Aptychus rugosus* was associated with *B. mucronata*"; further, no ammonite or nautilus is known in the zone of *B. lanceolata* at this locality, but *Haploceras leptophyllus*, which occurs at a lower zone, is associated with a coarsely tuberculate aptychus of very different appearance. From these facts Dr. Blackmore infers that the three aptychi mentioned above belong to no ammonite, but to the belemnites with which they are respectively associated.

As to the value and suggestiveness of these facts, there is no question. Even if we doubt the main conclusion of Dr. Blackmore, we receive from him a new guide for the determination of these zones in all parts of the country; and such facts can only be obtained by the local collector and stratigrapher, whom we are doing what we can to encourage and assist. But when we turn to the zoological inferences of Dr. Blackmore, we find the existence and intervention of the specialist fully justified.

Dr. Blackmore figures specimens of *Belemnitella lanceolata* and *B. quadrata* [we follow his nomenclature], as evidence that the lower margins of the aptychi fitted the upper margins of the belemnite-guards; but as to the imperfection of these specimens no one acquainted with belemnite structure can have any doubt. Then again, if it were possible to accept the homology of these aptychi with the pro-ostracum, as advocated by Dr. Blackmore, it would be impossible to regard their median line of junction as "probably a continuation of the ventral sulcus or the siphuncle"; for whereas these structures are ventral, the pro-ostracum, as the schoolboy knows, is dorsal. In any case, the whole idea that the pro-ostracum of a belemnite could become detached and form these twinned independent calcified masses, is foreign to every conception of the cephalopod shell, and would require substantiation by careful comparative studies.

Until some observer with Dr. Blackmore's care and enthusiasm shall have discovered, *in situ*, specimens of these belemnites and their supposed aptychi in juxtaposition, we do not think the suggestion demands serious discussion. Not that we refuse to admit the possibility of such a discovery; only we should expect to find these aptychi dorsal in position, not representing or replacing the pro-ostracum, but lying within it at its anterior end. They would, in fact, probably occupy the position of the nuchal cartilage in *Sepia*, a structure which, as Von Ihering long ago pointed out, bears a striking resemblance to an aptychus in form, and which, it may be added, bears to the shell and soft parts much the same anatomical relation as does the hood in a nautilus, or as the aptychus is presumed to in an ammonite. Such a discovery would not strengthen the case for the dibranchiate nature of the Ammonoidea; nevertheless, it would be of such interest that we hope Dr. Blackmore and others will do their best to confirm the novel proposition now put forward by him.

JAMES HALL.

FEW persons with any pretensions to geological knowledge are not familiar with the works of James Hall, for sixty years the State Geologist of New York. At the recent meeting of the American Association for the Advancement of Science, held at Buffalo, a special celebration was inaugurated in his honour, and the veteran geologist was present to receive the congratulations and acclamations of his friends. The celebration was opened by Professor B. K. Emerson, and Professsor Le Conte spoke on behalf of the Geological Society of America. Professor Hall briefly replied.

To give any sufficient account of the contributions to science by this remarkable man, would be to fill a number of this journal. We can therefore only briefly refer to some of the chief labours to which his long and active life has been devoted. In 1836 the official survey of

New York was organised under the direction of Mather, Emmons, Conrad, and Vanuxem, James Hall being appointed assistant geologist. Within a year the districts assigned to these men were changed and Hall was placed in charge of the Fourth District. Other changes brought him finally to the charge of palæontology; and in the elucidation of the fossil remains of the State of New York, Hall has therefore laboured for upwards of sixty years. The noble series of volumes issued by the State, and known as the *Palæontology of the State of New York*, will remain a great and permanent monument of the untiring energy of James Hall, the sole survivor of that little band, the first generation of American geologists. Fortunately for geology in general, the United States has ever been famous for the singular liberality with which she has distributed world-wide the labours of her sons, and Hall's works are as accessible to English geologists as are Lyell's Elements.

But it is not in palæontology alone that Hall has gained his laurels; while hard at work upon the fossils of the State, he was also engaged in stratigraphical matters, and found time to deal with the broader philosophical questions which lie at the root of the subject. He wrote brilliantly on the loading of the earth's crust and on oscillations of level due to denudation and removal of pressure, and on the origin of mountains and sedimentation. He mapped the State of New York several times, and worked away on the complex relations of the rocks until their secrets were fairly wrested from them. As active, apparently, at 80 as he was at 25, James Hall's figure could be seen in all weathers tramping the hillsides in sunshine or snow, never seeming to tire or flag under any difficulties, and it was in this present decade that one of us drove in his trap at imminent risk to his life, driven as it were by a young buck of 20, instead of a veteran of almost 80. And even more remarkable still is the fact that the Professor crossed the United States from West to East to take part in this celebration, and has once again taken the field in active service to the science he has loved so long, rather than seek his well-merited repose.

Professor Hall has gathered round him a large following of the younger men, who have rendered him great service in carrying out his plans, and he would be the first to give them his ungrudging acknowledgments. We are glad to offer him our congratulations, and indeed those of every geologist in this country, and to hope that he may long enjoy that health, which alone makes a multiplicity of years a great and pleasurable gift.

A full account of Professor James Hall's life and work, with portraits, will be found in *Science* for November 13.

The Eocene Beds of Dorset.

In the August number of the *Quarterly Journal of the Geological Society*, Mr. Clement Reid has given a sketch of these deposits, our knowledge of which has been greatly enlarged owing to the re-survey

of the western end of the Hampshire Basin. The London Clay thins to less than 100 feet in Dorset and becomes more sandy and pebbly, though still apparently of marine origin. The Woolwich and Reading series becomes more fluviatile than at Newhaven and Portslade, as it contains lenticular patches of subangular gravel to the west of Wareham. The Lower Bagshots also become coarser and more purely fluviatile westward, the change near Dorchester being singularly rapid. Mr. Reid's study of the composition of the Eocene gravels in this area shows him that the rivers which deposited them must have flowed from the west or south-west. The paper, of course, gives the details of which this note is the mere conclusion.

BOTANICAL SCRAPS FROM AMERICA.

WE have just received some portions of vol. x. of the *Proceedings of the Washington Biological Society*, containing small contributions to the botany of the mountain regions in the Western United States. C. Hart Merriam describes a new fir (*Abies arizonica*) from the San Francisco and Kendrick Mountains, Arizona, where it grows at an altitude of between 9,000 ft. and 10,000 ft. It is a tree about 40 feet high with fine-grained elastic bark, the creamy-white colour of which makes it a conspicuous object in the mountains. Its nearest ally is *Abies lasiocarpa*, a tree ranging from Southern Alaska and British Columbia southward, over the Rocky Mountains into Utah and Colorado, and over the Cascade Range to Southern Oregon. The smaller cones and broader cone-scales are distinguishing features of the Arizona fir.

F. V. Coville describes a new rush (*Juncus confusus*) from the Rocky Mountain region, and gives a synopsis of the group of (chiefly) American species to which it belongs. He also gives an account of a new currant (*Ribes erythrocarpum*) found growing near Crater Lake, Oregon. Crater Lake is described as "a remarkable body of the purest water, nearly circular in form, about six miles in diameter and 2,000 ft. in depth, without a visible outlet, occupying the bowl of an extinct volcano in the southern part of the Cascade Mountains of Oregon." The surface of the water has an altitude of 6,239 ft., and the surrounding cliffs rise 1,000 ft. to 1,500 ft. higher. The mountain slopes are densely forested, and as no botanist seems to have heretofore explored this part of the Cascades, the plants collected by Mr. Coville and his friend may prove of unusual interest.

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I.

Wanted, a British Fresh-water Biological Station.[1]

ALTHOUGH there are fresh-water biological stations actively at work in Germany, France, Switzerland, Bohemia, (see NATURAL SCIENCE, vol. v., p. 370., Nov., 1894), Sweden, Finland, and the United States, besides several in contemplation in various other countries, the necessity for the establishment of a fresh-water station in this country has received very little attention. In fact, with the exception of the proposal to found such an institution in Norfolk (see *Transactions* of the Norfolk and Norwich Naturalists' Society, Vol. vi., Part 1., p. 108., also NATURAL SCIENCE, Jan., 1896, p. 8.), the very idea of working methodically at the fauna and flora of our inland waters never seems to have been seriously entertained by our biologists.

There can, however, be no question that from the purely scientific point of view, a knowledge of the life-histories, habits, and inter-relations of fresh-water organisms is of equal importance with similar knowledge concerning marine organisms. It may even be of greater importance in some cases, for there can be little doubt that many of the fundamental biological problems can be answered (if answerable at all by a study of aquatic organisms) more easily by observations upon fresh-water than upon marine forms, because their conditions of existence are less complex. From the practical point of view the claims of fresh-water biology are, it must be admitted, not so great as those of marine biology, but they are by no means inconsiderable, and will tend to become stronger with every increase in the amount of attention paid to fresh-water fishes as food.

Surely it is time, therefore, now that the more pressing need for British marine biological stations has been largely satisfied, and the anticipations as to their value are being steadily realised, to consider if the careful study of fresh-water biology in this country cannot be helped forward by the establishment of a properly equipped station. Comparatively, no doubt, a greater amount of work can be done upon fresh-water animals and plants without the aid of a special station, than is possible in the case of marine forms, but it is nevertheless true that there remain a long series of problems in fresh-water biology which will never be solved without the continuity of observations practically only to be secured by means of a station definitely

[1] Paper read before Section D of the British Association, September 18, 1896.

working towards this end. And these problems are, for the most part, not the narrower specialist questions, but those of great general importance bearing on vexed points of variation, heredity, selection, and the influence of environment.

As to the position of such a British station, it would probably be well, for reasons of accessibility, to limit the possible situation to England and Wales, although the claims of several localities in Scotland and Ireland are not light. Undoubtedly the three districts in England and Wales offering most suitable conditions for the establishment of a fresh-water station, are the Lake District, North Wales, and the Norfolk Broads. The claims of the Lake District and North Wales are very much of the same character, though probably the former has a somewhat richer aquatic fauna and flora than the latter. In both cases some of the principal work to be done would be carried out upon the free-swimming organisms constituting the fresh-water "plankton." That the detailed study of these forms is likely to lead to important results may be seen from the following considerations. The number of species constituting the bulk of the plankton of fresh-water lakes is remarkably small, the conditions of existence are comparatively simple and uniform, and the certainty with which typical collections may be made is at its maximum. In fact, in studying the fresh-water plankton one gets, under perfectly natural conditions, almost the simplicity of an artificial experiment, and it is here, if anywhere, that some of the problems of selection, variation, and heredity will find a satisfactory solution. Many other lines of work besides observations on the plankton could easily be undertaken by a station in either of the two districts named, but the latter would still form its characteristic feature. The Norfolk Broads and rivers, on the other hand, offer little opportunity for plankton work, but possess the unique character of presenting continuous and nearly imperceptible gradations from absolutely fresh to brackish, and even salt water. This is a condition, the influence of which has never been sufficiently studied, and has certainly never been touched by any existing biological station. That this peculiarly favourable example of gradation from fresh to salt water is leading to important changes in the habits of various species is almost certain, and it has already been shown for one group of animals, viz., the Ostracoda (see Brady and Norman's Monograph of the Ostracoda, &c., Royal Dublin Society's *Transactions*, 1889 and 1896), that several species, which are typically brackish (*Cypridopsis aculeata*, *Cythere pellucida*, *C. fuscata*, &c.), are now living in broads and dykes that are quite fresh, whilst other forms, essentially marine (*Cythere antiquata*, *Loxoconcha guttata*, &c.), have succeeded in pushing their way into the brackish waters of the district. These facts suggest a subject for systematic observation which should be taken up by a biological station on the Norfolk Broads as one of its most important duties. It is certainly not too much to hope that in this way some very important additions to our knowledge of the

origin of fresh-water faunas would be made in the course of a few years. As in the case of a station in the Lake District or North Wales, in addition to the more characteristic work determined by the nature of the locality, a considerable number of valuable investigations in fresh-water biology of a more general nature could be carried out on the Norfolk Broads, and it is also to be borne in mind when discussing the utility of a fresh-water biological station, that, wherever it is situated, the mere working out of the aquatic fauna and flora of the immediately surrounding district, which is almost essential as a preliminary step to deeper investigations, would be in itself no small gain to science.

The cost of a fresh-water biological station would depend somewhat upon whether it was floating or fixed. A floating station would no doubt be cheaper in the first instance, but a fixed station offers much greater accommodation for workers, and also greater facilities for experimental investigations. The minimum initial cost, however, even of a floating station, could not be much under £500, and in addition to this it would be absolutely essential to provide for the salary of a trained biologist. If the undertaking is to be in any measure a success, there must be one man at least to live and work continuously at the station. Anything less than this would prevent the continuity of endeavour which alone lifts such a station above the level of an attempt to catalogue the local fauna and flora, an undertaking which, however praiseworthy in itself, is not likely to receive much more than local support.

Compared with the large sums spent on marine biological stations, the amount required for a fresh-water station, even if provided with a little more than the minimum outfit, is very modest, and it is scarcely necessary to advocate the formation of a special society to carry through the proposal to found such a station. At any rate, it seems clear that in the present state of public opinion on the question of the value of fresh-water fishes as food, such an association could not be formed on the broad basis of the Marine Biological Association; but the work could probably be accomplished without the aid of a new society. A little co-operation on the part of the many existing Institutions interested in biology, with a local society willing to undertake the work of organisation and supervision, seems to be all that is required. At least, so far as the establishment of a station on the Norfolk Broads is concerned, this method would be sufficient, for there is the proposal of the Norfolk and Norwich Naturalists' Society already in the field, and it would be a great pity if, from want of support, a scheme should be allowed to fall through which, if carried out, would not only add materially to the general stock of scientific knowledge, but would also remove the reproach that the United Kingdom is almost the only country in Europe without a fresh-water biological station.

Fillebrook Road, Leytonstone. D. J. Scourfield.

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II.

The Position of Morphology in Zoological Science.[1]

A PAPER which proposed to insist on the cardinal value of morphology in the science of zoology, would have been held some years ago to be an entirely superfluous statement of a universally recognised truth. It cannot, however, be denied that there has been growing up amongst zoologists in the past ten years a feeling of dissatisfaction with morphological methods, and a tendency to disparage altogether morphology as a means of research. This feeling finds expression, for instance, in Ray Lankester's article on Zoology in the "Encyclopædia Britannica," where we read that "pure morphography has long since ceased to be a principal line of research," and that the attention of young students should not be confined to "what are now, comparatively speaking, the less productive lines of research." In Bateson's "Materials for the Study of Variation" we meet the broad statement that the morphological method has failed. Driesch and others who work on the same lines estimate the value of morphology at a very low figure.

I propose in the present essay to examine briefly the rival methods which have been put forward as demanding chief attention from zoologists, and shall show that all of them, valuable as they undoubtedly are, suffer from defects from which morphology is free. I shall further inquire to what causes the feeling of discontent with morphological methods is due, and finally I shall tentatively indicate certain ways of dealing with morphological facts which seem to me likely to be free from the objections which have been raised to other methods, and promise to throw fresh light on the general problems of zoology.

The lines of research which have been put forward as being more important than morphological investigations and comparisons, are mainly three, viz., Experimental Embryology, Study of Individual Variations, and Statistical Studies in Variation, or Mathematical Zoology. All are avowedly attempts to solve one of the chief problems of zoology, viz., the nature and causes of animal change or variation. Experimental embryology founded by Roux and carried on with such success by Driesch, Hertwig, and Loeb, has for its aim the isolation of the various processes the conjoint working of which con-

[1] Paper read before Section D of the British Association, September 22, 1896.

stitutes ontogeny. It is therefore to be looked on as a new kind of dissection. Many embryologists have attributed to the visible units observed in a segmenting egg, independence of growth and a definite function in building up the organism. The experimental embryologist, however, by forcing these units into abnormal positions, or by isolating them, is able to demonstrate that such attributes are impossible, and that the real factors into which development is separable are not identical with the visible differentiation which the germ exhibits. Driesch distinctly states that this dissection is the whole aim of experimental embryology, but some of his followers seem to imagine that by such means it is possible to discover the ultimate causes which bring about variation. This idea is, I think, a totally mistaken one. No examination of the changes which, by the application of physical or chemical means, we are enabled to bring about in the outer structure of the organism, will bring us one step nearer the discovery of those causes which are able to modify its inner hereditary potentiality. We may, like Driesch, force that part of the egg which normally produces the head to give rise to the tail, or, like Herbst, turn endoderm into ectoderm; all we arrive at is a resultant of the combined working of hereditary tendency and effect of environment.

The study of individual variations stands on a different basis. The modifications which species undergo are *ex hypothesi* made up of a summary of individual variations, and it seems quite proper to begin with a consideration of these; but at the outset we are met with a most serious difficulty. We have no means of distinguishing modifications, which have been produced by the action of environment on the particular individual we examine, from those due to variations in its hereditary qualities. Still further, the individual variations which appear most prominent to us are often those which there is strong reason to believe have never taken part in the evolution of species. In his "Materials for the Study of Variation," Mr. Bateson has collected numerous instances of supernumerary digits in Vertebrata and of branched legs among the Insecta; yet, in all the hundreds of species of Mammalia, and the hundreds of thousands of species of Insecta, no solitary instance is known where a variation of this kind has become the distinguishing mark of a species. Even of those changes due to variation in the hereditary powers, it is clear that only a small proportion concern us. For, in order to become a real factor in evolution, it is necessary that a variation should not only be transmissible to the offspring, but further, that it should occur sufficiently frequently to give natural selection an opportunity of taking advantage of it.

The Statistical Study of Variations, which we owe to Dr. Galton and Professor Weldon, cannot, I think, be assailed on theoretical grounds. This method aims at representing in a curve, not only the extent of variation in a given character met with in a

species, but also the number of individuals exhibiting any particular variation. We can thus see at a glance the proportion between the average and the departures from it, and if we should find, by comparing curves constructed from the examination of individuals of different ages, that the death-rates of the possessors of different variations were different, we should have reasonable ground for supposing that natural selection was weeding out certain variations. We should further have an answer to the question whether the species was being modified or not—this answer being positive or negative according as the death-rate was greater on one side of the average than on the other, or equal on both sides.

The main objections which can be urged against this method are of a practical nature. First, we isolate a character, and try to determine whether or no its possessor suffers by its presence. But "characters" are mere mental conceptions, they do not exist by themselves, and natural selection acts, so to speak, on the balance of all the characters. Of course, if we could prove that individuals possessing a certain character had a lower death-rate than individuals without it, we might expect that that character would become a distinguishing feature of the species, though the survival of those that possessed it might not be due to its utility, but to some constitutional peculiarity of which it was the by-product. How are we to determine such an association of death-rate and variation? The attempt has been made by drawing a curve for the same character in the case of young and of older animals. The curve was flatter in the case of the younger animals; that is, the number of deviations from the mean was found to be greater in them: hence it is assumed that these abnormal forms are continually being weeded out. It is possible, however, that there may be a self-regulating tendency in growth. Are not abnormalities observed in children often toned down in later years?

There is another objection, the validity of which would not be admitted by many, but which seems to me to have a certain weight, and that is this: So far as we can discover, the condition of the organic world has remained relatively stable during the historical epoch, and no new species are known to have been formed. It is a question, therefore, on the one hand whether the conditions which brought about the great variability of which we see evidence in the geological record, still persist, and if not, whether the process of evolution be not so slow, that the infinitesimal period over which our observations can extend gives us no clue as to its direction.

From the standpoint of morphology it seems to me that we are free from all these difficulties. We take as our units not individuals but species, and if the doctrine of descent be admitted, we must infer that specific characters have themselves been of importance in the struggle for existence, or have been inextricably bound to others which have. If specific characters then are our material, certainly they offer a sufficiently wide field to work on and give us a unit small

enough to start with, since there are many specific distinctions far less obvious than many individual variations. Many specific characters are doubtless merely associated with other characters which really determine the survival of their possessors, but this is true to a less degree of generic characters, and as we pass over to family and ordinal groups we get on safer and safer ground in assuming that we are dealing with features directly due to natural selection.

Why then has this feeling of dissatisfaction with the method of morphology grown up? The main reason is, I think, a conviction that it proves too much. The most discordant views as to the relationship of animals and their ancestry have been drawn from the same facts, and there does not seem to be any court of appeal before which rival views can be brought. Then again, continued study has forced home the conviction, that the processes of evolution are much more complex than was at first imagined, and that so far from being a simple process from the less to the more differentiated, the converse, viz., *degeneration* or simplification of structure, is also going on; further, that similar structures are sometimes independently developed along different lines of descent, in virtue of what was called "parallel" or "convergent evolution," but termed by Professor Lankester *homoplasy*. Now the discovery of the great principles of degeneracy and homoplasy, whilst it explained many points, has caused considerable doubt as to the certainty of morphological reasoning. For really, when armed with the principle of progressive degeneracy as well as with that of progressive differentiation, there is no limit to the powers of the evolutionary theorist; one can derive literally any one animal from any other by first deleting all the obnoxious organs in the supposed ancestor, and then evolving any number of new ones. Then again, if similar structures may have been developed independently in two different stocks by the action of similar external conditions, where is one to draw the line? How far is one justified in relying on similarity in structure as a criterion of community of descent at all? These, I think, are some of the questions which have underlain the feeling of scepticism as to the value of morphology which has crept over many zoologists, and which have caused, I confess, much trouble and distrust in my own mind. I now venture to suggest ways of looking at morphological facts, which seem to me more fruitful than the ordinary methods, and which have given me fresh hope in the pursuit of morphological study.

First, it is a mistake to assume that in tracing a supposed line of descent we are at liberty to assume that any conceivable variation may have occurred, variations, for instance, the utility of which we are not bound to explain. I think if we take specific and generic distinctions as our units, we are bound to show that some parallel change to the one we postulate has in all probability taken place. Thus there has been an immense amount of discussion as to how the pentadactyle limb was derived from the fin of a fish; but no one, so far

as I am aware, has examined with care the cases of those Teleostean fish, like some species of blenny, which come out of the water and seek prey between tide-marks, where a fin has been to some extent transformed into a supporting limb. I do not wish to dogmatise against the possibility of changes having taken place in past time altogether different in kind to those of which we find evidence in present specific differences; but I do maintain that if we assume such changes in explaining the present structures of animals we are on utterly unsafe ground. My suggestion then is that by carefully comparing and tabulating specific and even generic differences we may be able by induction to arrive at "laws of successful variation." An example or two will serve to make clear what I mean. If we review the group of the Lamellibranchiata, we are struck by such forms as *Teredo* and *Pecten*. *A priori* it is possible to argue that the idea that these were derived from the ordinary type of Mollusca is hypothetical. Practically all zoologists are agreed about it, no different explanation having so far as I know ever been suggested. Now my point is that cases like these give us definite data to go on; they really enable us to ascertain now,—what Bateson has looked forward to doing only in the remote future—that certain changes are possible to certain animals. Perhaps it may be retorted that all this is known and acted on. My reply is that many changes have been postulated, which have no analogue amongst variations that we know must have occurred. Balfour postulated the formation of a new mouth; Dohrn that of a new arm. All through the Mollusca we find no such fundamental change, nor the foundation of any new type skeleton such as the supporters of the Annelid theory are bound to postulate.

If we now turn to the difficulty of distinguishing primitive and undifferentiated from degenerated structures, we shall find, I think, that objections may be made to the current modes of reasoning on this subject. It is often implicitly assumed (1) that if an animal can be shown to be degenerate in one respect, it is not primitive at all, and throws no light on the ancestry of a group; (2) that any amount of degeneracy may be followed in the history of the race by any amount of forward evolution. I shall give reasons for questioning both suppositions. To prove that the first is a real factor in theorising, we have only to remember the war waged round *Amphioxus*. Few would deny that it is degenerate in some points; the question at issue is, Does it in its general organisation represent the ancestor of Vertebrata or not? Now it seems to me that on *â priori* grounds we have every reason to expect that all animals, which possess on the whole a primitive organisation will be degenerate in some features. For their modified relatives which have departed from the ancestral type have *ex hypothesi* been forced to do so by natural selection, and how have these primitive animals been able to escape from that pressure of environment which modified allied forms? In some cases, perhaps, isolation on oceanic

islands, or in small lakes may have allowed them to do so; but in the case of what we may term phylum-ancestors such as *Amphioxus* and *Peripatus* such explanations are futile. The grade of structure they present, dates from such a remote epoch that no physical barrier will have remained constant in the interval, added to which their wide distribution at once negatives such an idea. No; the general method in which such animals escaped from the stress of environment, was by taking to burrowing or sessile modes of life, and this has inevitably carried a certain amount of degeneracy with it. The idea that active vigorous vertebrates are descended fnom a sluggish mud-eating worm like *Balanoglossus* must strike many people as highly improbable, but that *Balanoglossus* is descended from a free swimming form with well developed eyes we know from the structure of its larva to be extremely probable, and there is nothing violent in the supposition that this same form may have given rise to the Vertebrata. I think, if once the dictum, "All primitive animals are also in some degree degenerate" were accepted, a harmonious explanation of many discordant facts would be attained.

In this connection I may say that such phrases as "degree of modification" are in need of definition. I think many zoologists have admitted there are two kinds of modification at work: first, in the intensity of metabolism or the degree of vitality, by which we mean the degree of specialisation of the organs for carrying on the vital processes—digestion, respiration, circulation, excretion, &c.; and secondly, in shape and form of the outer appendages, or of the general shape of animals. Now, it is the first kind of modification to which zoologists attach weight. What we desire above all to know is how the complicated physiological mechanisms represented by the higher vertebrates have been built up. It is assumed that variations in form and size—the outgrowth of flaps and lobes, &c.—are things which may have been accomplished in a comparatively short time. When, therefore, zoologists speak of animals being primitive, they mean with regard to internal organisation. On grounds of the relatively greater importance of the latter, they separate the beaver and wombat, the great ant-eater and the pangolin, &c., &c. This distinction between internal and external structures is sometimes explained by saying that the latter are adaptive, as if all characters were not ultimately adaptive. Explicitly stated, the belief underlying such principles of classification is: Improvement and elaboration of internal structure and constitution is a slow process; modification of external structure is a rapid one, and has given similar results again and again. The Brachiopoda are a good instance of a group where the external form is highly specialised, but where inner differentiation is at a very low ebb. They exhibit a most primitive condition of the excretory, generative, circulatory, and nervous organs. These animals form a part of the oldest fauna known to us; but if the view which I have been trying to propound is correct, the

conclusion that the date of the appearance of this fauna is enormously removed from the beginning of life, because it contains highly specialised forms, must be taken with considerable abatement.

Turning now to the question whether we can legitimately assume that degeneracy and differentiation have alternated in the history of a species—in a word, whether degenerate ancestors have given rise to active forms with highly developed sense-organs—let us glance at those cases where all will admit that a certain amount of recovery has occurred. One of the best-known cases is that of *Pecten*. There is strong reason for believing that all lamellibranchs are descended from more active forms provided with cephalic eyes and tentacles, and separate cerebral and pleural ganglia. Owing to their sluggish, burrowing mode of life, and gross non-selective feeding, these features have been lost. The oysters, to which *Pecten* is not distantly allied, have carried this degeneracy to its extreme point—the foot, one adductor muscle, and the pedal ganglia being likewise absent. *Pecten* has recovered the free-swimming life, and has developed a new set of sense-organs; but none of the lost structures have been recovered. I know of no other case where it is at all probable that recovery has taken place, and there is no ground for assuming that burrowing forms could ever give rise to active descendants. This brings us to the question, how primitive or undifferentiate can be distinguished from degenerate features. How are we to say, for instance, what features in *Amphioxus* we may regard as ancestral, and what as secondary; or in *Peripatus* or *Chiton*? I do not assert that any absolute criteria can be put forward; each case must be judged on its merits: but certain suggestions can be made. The criterion of primitive characters is their synthetic character—that is, they serve to link together different groups. Why are the gill and foot of *Nucula* said to be of a primitive character? Because they agree, in contradistinction to those of the lamellibranchs, with the gastropod gill and foot. An organ may be said also to be primitive if it combines functions distributed in other animals amongst different sets of organs. Thus, the cœlom of brachiopods combines the functions of excretory organ, generative organ, and body-cavity: so we regard it as being in a very primitive condition. Underlying such reasoning there is a principle I believe to be sound; it is that new organs are never developed from functionless rudiments—as Darwin at one time thought himself forced to believe—but arise by modification of pre-existing organs. Where the function of an organ is changed, the newer function must have existed with, but subsidiary to, the old one. Eyes and ears would thus be local spots of peculiar sensitiveness on a skin which responded to both light and sound. Degenerate organs are no doubt simplified, but not only are they not synthetic in the sense mentioned above, that they do not recall the conditions of affairs in other groups, but they are not correlated with the state of development of other organs. A

good instance is found in the limbs of perennibranchiate Urodela. These are, it is true, constructed on the pentadactyle plan, but they are exceedingly feeble, and the number of digits is in almost every case reduced. They are thus unable to support the weight of the body, the purpose for which the pentadactyle limb was originally evolved, nor do they, on the other hand, show any of the characters of fins. Hence, we conclude that perennibranchiates have been derived from caducibranchiate forms—forms, at any rate, which walked more and swam less,—and the life-histories of *Siredon* and *Menobranchus* have now proved that we are right. Then, again, in the case of the head of *Amphioxus* we notice that it is asymmetrical, and that the brain and sense-organs are almost entirely obsolete. But we know that the whole organisation of Vertebrata is permeated by bilateral symmetry, and that they all have well-developed brains and sense-organs, and lead typically a free, roving life. To this life only can we attribute the fact that *Amphioxus*, like other vertebrates, is flattened in a vertical plane, since only in swimming could a body of such a shape be easily maintained in equilibrium; asymmetry and feeble sense-organs are inconsistent with such a life, and hence we interpret them as secondary features, due to the secondarily-acquired burrowing habits.

One of the most vexed questions in zoology has been the value of the evidence afforded by embryology. The older anatomists roundly asserted that the ontogeny of the individual was a recapitulation of the phylogeny of the race. They endeavoured to find ancestral meanings for all the embryonic structures which they had observed. Lately it has become fashionable to look coldly on such theorising, and some have even gone so far as to deny that there is any evidence that ontogeny is in any sense a repetition at all. I cannot but think that the latter class of zoologists are in much the same position as the theological opponents of the doctrine of evolution —they are most imperfectly acquainted with the facts. It must be remembered that comparative embryology is only an extension of comparative anatomy; that it is most arbitrary to say that only sexually adult forms are to be compared with one another; and that the conclusion that resemblance between the immature stage of one animal and the mature stage of another is indicative of affinity, is precisely on all fours with a similar conclusion drawn from a comparison with one another of two adult stages. But there are many cases where no one really doubts that the affinity indicated by ontogeny is correct; in other words, that in these particular cases ontogeny is a repetition of past history. Such cases are the pentacrinoid young of *Antedon*, with the conspicuous basals and orals, the cyclops-like larva of the parasitic copepods, the tadpole-like larva of the ascidians, and, in general features, the tadpole of the frog. Now, if there be this undeniable hereditary basis for ontogeny in some cases, it is exceedingly unlikely that it is a factor which is only sporadically

present. The process of development of all animals from the egg is in all probability fundamentally the same kind of process in all cases, and made up of the same factors, though some may be more prominent in some cases than in others.

Of course, everyone admits that there are numerous features in ontogeny which are not due to ancestral repetition, but to subsequent modification of the larva, and the question is how are the results, due to these two factors, to be distinguished from one another. Now, an important step towards attaining this result has been made by Mr. Sedgwick in his theory of the relation to each other of the embryonic and larval types of development. This theory is briefly as follows: An embryonic stage of development is nothing but a larval stage, which has been sheltered from the external world, by either being enclosed in an egg-shell or retained in the body of the mother, and modified in consequence. The larva retains ancestral features, because it is subjected to ancestral conditions of life; when, however, as in the case of reptile ontogeny, it has become converted into the embryonic type, then a change, such as the loss of limbs, when it occurs, can affect the development as a whole, and not even embryonic vestiges remain, as is the case with snakes. On the other hand, so long as the larva of the frog lives in water, it must retain many fish-like features. I think, however, we should make a mistake if we limited the influences tending to retain ancestral structure to the outer conditions, when these latter happen to be of an ancestral type. It is to me impossible to suppose that the general form of insect larvæ, the vermiform shape, and the large number of almost similar segments, is not an ancestral feature; but in view of the extraordinarily diversified habits of these larvæ, we cannot suppose that the influence of outer conditions has retained it. If, however, we say that the larval form is the combined result of outer and inner conditions, we shall, I think, be nearer the truth. By inner conditions I mean the intensity of the metabolism of an animal, correlated with which is the differentiation from one another of the organs fulfilling the various functions; such a level of metabolism as the adult possesses being only gradually obtained, when the said level is a comparatively high one. Thus, when the long and comparatively undifferentiated nervous system of the caterpillar becomes converted into the concentrated nervous ganglia of the butterfly, there is no doubt that we have reached a higher level of life.

Suppose, then, that the ancestral features of ontogeny are due to the repetition of ancestral outer and inner conditions, we can make a rough estimate of characters which will *not* tend to be preserved by these causes, but which, for the most part, will be obliterated by subsequent modification. Chief among these is *size*: most larvæ are comparatively minute; only in a few cases does the larval stage rival in size the adult, while in very few cases (urodele amphibians) it tends even to supplant it. Now, along with reduction in size goes, in all

probability, reduction in number of a series of homologous organs performing the same kind of function, since there is little doubt that the repetition of members is a phenomenon of fundamentally the same nature as vegetative reproduction, and that it is, primarily at any rate, correlated with growth in size. Thus, we can understand why the nauplius larva should lose the long series of post-oral parapodia-like appendages which we have reason to believe the annelid ancestor of arthropods possessed, and should retain only those first three modified ones, the modification of which was probably one of the chief changes by which an annelid was converted into an arthropod. A most interesting confirmation of this view is obtained by comparing the nauplii of the various crustacean groups. The most annelid-like order is that of the phyllopod Branchipoda, and here the nauplius still retains traces of the post-oral segmentation. In the other groups, notably the cirripedes and the ostracods, the nauplius has lost all traces of this segmentation, and is obviously secondarily modified, since in each case it shows precociously some features of the adult.

The general result of this way of regarding ontogeny will be that we shall regard no larva as purely secondary in all its features, and that where we compare two animals more or less allied, we shall attach the greatest importance to the ontogeny of that one which has the longest larval history. It is, so far as I know, a rule without any exception that when we compare corresponding stages, what is obscure and difficult to interpret in the embryo, becomes clear and instructive in the larva. The Echinodermata have perhaps the longest larval history in the animal kingdom; they begin free life as blastulæ, and in no group is the process of the formation of the primary germinal layers so diagrammatically clear.

Finally, we have to consider the greatest difficulty of all those involved in morphological reasoning, namely, that which the recognition of the principle of homoplasy brings with it. The proposition that similar organs might arise in different animals under the stress of similar conditions, seemed at first to offer merely a convenient solution of some difficult problems, so long as the organs in question belonged to members of widely different families, such as the tracheæ of insects and arachnids for example. It is, however, becoming every day clearer, that such parallel evolution has taken place again and again within narrow circles of affinity. Let any morphologist examine the structure of a large group; let him by careful comparison of the various species with one another, arrive at an idea of the primitive form from which all were derived, and then let him attempt to indicate the lines along which the different forms have been evolved. He will find himself driven to the conclusion that important structural features on which he would be naturally inclined to found his system of classification, have been developed twice or thrice. Numberless instances of this might be given. Calcareous sponges used to be divided into three well-marked classes, according to the character of

their canal system, viz:—the ascons, the sycons, and the leucons, forming three stages in an ascending scale of differentiation. Now Sollas and other competent spongiologists consider that the leucon type has been evolved many times amongst the different families into which the sycons are divided. The horny sponges and the sponges devoid of spicules have likewise had in each case two or three roots. Among the echinoderms we meet with similar cases on every hand. One of Perrier's main divisions of the asterids is that of the Valvata, in which the plates which form the skeleton are covered with a uniform granular coating, there being practically no spines. This division includes long-armed and short-armed forms; the latter glide by insensible gradations into the short-armed forms of the group Paxillosa, in which the armature is composed of circles of spinelets borne on a button, of which *Astropecten* is the best known example. The long-armed forms probably have no affinity with Paxillosa. The term "short-armed" is not quite correct; it is not only that the radii are comparatively short, but that the arms have coalesced with each other laterally. Amongst the ophiurids, the habit of carrying the young in the genital bursæ, which gives rise to great differences in the development, has been independently acquired in four or five distinct genera, other species of which give rise to free-swimming larvæ. Among echinids the eccentric position of the anus, and the peculiar modification of the ambulacra, involved in their assuming the petaloid form, when the tube-feet become broad respiratory leaves, is found in the Clypeastridæ and Spatangidæ. As the first group retain the jaws of the regular echinids whilst the second have lost them, one might at first sight infer that the spatangids were merely a further development of the clypeastroid type, but this supposition is precluded by the fact that in both groups we find fossil forms with perfectly normal ambulacra and almost central anus. Among the annelids, the group of the tubicolous annelids is now divided up into families which are placed with families of the Errantia. Amongst arthropods, those air-breathing arachnids which retain lung-books, the Pedipalpi on the one hand and the scorpions and spiders on the other, have, according to Laurie, been derived from the water-breathing eurypterids along two distinct lines. The land-crabs, which have so completely acquired the air-breathing habit that they die when immersed in water, have been derived from various marine ancestors. The molluscs offer perhaps the best examples of all of homoplasy. The pteropods, once supposed to be a most clearly defined order, are now admitted to have been derived along two lines from the opisthobranchs; the Thecosomata from *Bulla*-like forms, and the Gymnosomata from nudibranch genera. In the newest classification of the Pulmonata, the two common slugs *Arion* and *Limax* are separated from each other, the first is classed in the same family as *Helix*, and the second is made

[1] M. Laurie, "Anatomy and relation of the Eurypterida." *Trans. R. Soc. Edinburgh*, vol. xxxvii., pp. 509–528, 1893.

the type of a family by itself. Amongst the vertebrates we find numerous examples wherever we choose to look. The Teleostei are polyphyletic, and so are the ganoids; so marked does the parallelism in evolution seem to have been, that our ideas as to the mutual relationships of the two groups are in hopeless confusion. Snake-like reptiles have been derived from many families of lizards. Huxley arbitrarily selected the characters of the absence of all trace of a pectoral girdle, and of an allantoic bladder as a criterion to determine the true snakes; but it is obvious that this is a mere makeshift. Among birds, parallelism is seen everywhere, and instances of it can be found in the works of Gadow and Fürbringer.

Turning finally to the Mammalia, we find Sir William Flower writing thus of the Ruminants:—"The great difficulty which all zoologists have felt in subdividing them into natural minor groups arises from the fact that the changes in different organs (feet, skull frontal appendages, teeth, cutaneous glands, &c.) have proceeded with such apparent irregularity and absence of correlation, that the different modifications of these parts are most variously combined in different members of the group."

Such facts as these are apt to have a disheartening effect on the student of morphology. If we try to analyse the feeling of disappointment to which they give rise, we shall find, I think, that it is due to a theory we are accustomed to assume as the base of our speculations on phylogeny, with which such facts are irreconcilable. This theory, rarely explicitly stated, but everywhere postulated, holds that when one large natural group of animals was derived from another (as for instance Amphibia from Pisces), this took place by *one species* of the lower group acquiring new characters and taking to a new method of life. It is then imagined that all the species of the higher group have been derived from the modification of this single ancestral species. The view suggested, however, by the ever increasing number of cases, in which we are forced to assume a parallel development, is that a complete homology or homogeny, and a homoplasy are only after all extreme terms in a series, in which the successive terms are very closely related to each other. We seem driven to the conclusion that when a large natural group of animals was being evolved, the changed environmental conditions, which were causing the evolutional progress, acted not merely on one species but on species belonging to the same or different genera, families, or even orders, and induced similar modifications in them. The course of evolution, therefore, instead of being represented by a single trunk of a tree repeatedly branching—the typical form of the Haeckelian genealogical tree—ought rather to be pictured as a column of parallel stems with interlacing branches, like the stipe of a mushroom.

The study of systematic zoology in fact suggests, that, given a definite set of environmental conditions, any species having a given general structure exposed to them, will undergo the same change.

On the Lamarckian hypothesis this result would be attributed to the direct action of the environment; on the Darwinian view to natural selection, the only assumption which it is necessary to make, being that the small variations on which selection acts may always be trusted to occur.

All admit that the larger differences in structure which separate animals are useful, that is, are adaptive in character. Most systematists are beginning to admit that such modifications may have been effected again and again, and in despair they are taking refuge in minute peculiarities of form, pattern and arrangement, which they hope are not adaptive. This seems to me to be a futile position, the provisional acceptance of which is only rendered possible because, except in the cases where closely allied forms follow each other in immediately superposed strata, we have no means of determining the exact ancestry of a species. What we really do when we determine the structure of an animal is to unravel a series of superposed adaptations, and we class together those animals in whose structure we detect evidence of their having undergone the same series of modifications in the same order.

All similarity in structure between two animals is primarily due to similar external conditions; the longer the period of action of the same environment has been, the more complete is the likeness; and, by identity of ancestry we can only mean the extreme case in which the action of similar environmental conditions has extended for an indefinite time back into the past.

Just, therefore, as it would be a hopeless task to attempt to trace a single hypha in a mushroom stipe back to the spore from which it arose, so it appears to me is the attempt to trace several modern species back to a single original species.

The important thing to know about any hypha is its relation to the general anatomy of the plant, and especially its level on the stem; and the important thing to know about the arthropod groups is not whether they were all descended from the same species of annelid, but whether they were derived from annelid ancestors by the same series of modifications. The morphologist should aim at establishing and defining definite grades or levels of structure, and correlating these with the environmental changes which produced them.

I do not flatter myself that any of the points of view which I have endeavoured to set forth in this paper is absolutely new. Each of them will be found to have been either explicitly stated or implicitly assumed by some zoologist or other; but there is perhaps no zoologist who has not argued in a manner inconsistent with some one of the principles I have endeavoured to establish; and it seemed to me most desirable to try to give a coherent and explicit account of the principles vaguely recognised and imperfectly understood on which phylogenetic speculation is based—and such an attempt has been made in this paper.

E. W. MACBRIDE.

III.

The Earliest Known Seat of Learning in Europe.

A REMARKABLE discovery relative to the culture of pre-historic times has recently been published by Ed. Piette in the third part of his series of "Études d'Ethnographie préhistorique" in *L'Anthropologie* (tome vii., no. 4). In the cave of Mas-d'Azil, on the left bank of the Arise, in the Department of Ariège, the lower layers indicated the previous existence of a cold, damp period; above them were found deposits characterised by bones of the reindeer, red deer, aurochs, horse, common bear, &c. Many of the antlers, and harpoons made from the bones of reindeer, were engraved. Next came a layer, having a maximum thickness of 65 cm. (2 ft. 1½ in.), containing a large number of pebbles, which had been painted with peroxide of iron. The coloured pebble layer, marking the Asylienne period, was followed by a snail-shell layer (*Helix nemoralis*). It contained remains similar to those in the previous one, and belonged to the Neolithic age, but in neither were polished axes found. That the climate was damp may be inferred from the great numbers of this species of snail. Polished axe-heads were found at a higher level in the Pélécique layer, which was also characterised by the presence of *Helix hortensis*, thereby indicating drier conditions of climate.

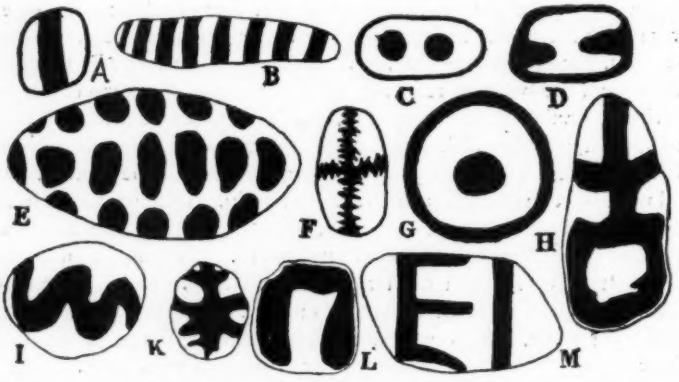
The humidity of the climate during the Cervian period was continued to a less extent during the Asylienne; the presence of trees shows that the climate was ameliorating at the time when the shell-layer was formed. Vegetation must have flourished under the influence of the warmer, though still damp, atmosphere, and men successfully cultivated several species of fruit-trees.

The Transition period, which followed the glacial period, began when the modern fauna had replaced the glacial fauna; that is, after the Equine or Eburnéene Period. The Transition Period had three phases: (1) the Cervian, during which the Quaternary industries continued, the implements remained unchanged, and the art of the reindeer-hunter was perfected; (2) the Asylienne phase, or period of the coloured pebbles, which came after the disappearance of the reindeer: by this time man had forgotten the arts of engraving and carving, but took to painting stones; (3) the Shell phase, marked by cinders mixed with snail-shells, and remarkable for the richness of its vegetation.

We can now turn to the coloured pebbles themselves. These are usually rounded, oblong and flattened water-worn stones of quartz or schist, which have been very rudely painted with red. Sometimes the whole surface is coloured, but more frequently a stone is marked on one or both sides with simple devices; occasionally the edge of the pebble is coloured so as to form a kind of border to the decoration.

Mr. Piette classifies these markings under four headings, viz.:—(1) numerals, (2) symbols, (3) pictographic signs, and (4) alphabetical characters.

Numerals: These markings may be straight lines which run across a pebble (A, B), or may be irregular rounded marks (C, D, E). No stone has as yet been found with more than eight bands (B). Mr. Piette considers the spots as representing units of higher groups, either nines, or more probably tens. In this connection, he points out that the Egyptians used strokes up to nine; ten was represented by a curve, the tens were grouped like the units; a hundred was indicated by



DIAGRAMS OF A DOZEN PAINTED PEBBLES, traced and reduced by one-third from the figures which illustrate the text of Mr. Piette's paper.

a spiral. There may be numerous spots on a pebble, one being found which had as many as twenty-three. In some stones a spot or blotch arises from the margin of the pebble (D). The author points out that this mark may also have a different value, and suggests that it may be the square of a higher-grade unit; thus a stone with twelve marginal blotches and six central spots (E) is credited with indicating a total of 1260 in the decimal system and 1728 plus 60—or 1788 on the hypothesis of a duodecimal system! This rather staggers Mr. Piette himself, and so he makes the admission that perhaps the differences between the marginal or tangent ovals and the isolated spots have no significance. Then, recalling the fact that a disc has throughout all time been employed to represent the sun, he asks whether they may not be signs in a hieratic writing, or units adopted by great men of the tribe, or even used to denote special

objects. The author certainly does not fail in ingenuity when he proceeds to throw out the hypothesis that these may have been counters in some game, or that they were painted to teach arithmetic.

Symbols: The occurrence of cruciform markings (F, H) naturally suggests sun-symbols, especially when they are inscribed within a border, a disc surrounded by a ring (G) being claimed as pictograph of a sun-deity—even the tau, or T cross, is represented. While admitting with the author that symbolism is a "perilous study," it would be too much to expect an enthusiastic archæologist to refrain from making the most of his material, and so Mr. Piette cannot be blamed for doing his best to read a meaning into these rude designs.

Pictographic Signs: Certain sinuous lines (I) are supposed to be pictographs of snakes. The author found in the Pyrenees a long wavy line of unhewn stones sunk in the ground, and resembling a serpent, with some granite blocks for a head, and near the bends was a beautiful group of cromlechs, in which he found neolithic remains. He also records two interesting experiences of his own, which prove that the cult of the serpent is not yet extinct in the Pyrenees or in the Department of Aisne. It requires some imagination to see trees, as does Mr. Piette, in some of the markings (for example, in K in the accompanying figure); some ovals, also, with central dots are called eyes, and certain elongated markings he names reeds.

Alphabetical Characters: A third of the paper is taken up with a discussion of markings (L, M) that recall the characters of primitive writings; and when we remember the recent discoveries of Mr. Arthur J. Evans in Crete, it is wiser to treat these markings with respect. At times, as in the case of the numerals, Mr. Piette rather gives himself away, but his discussion of these supposed script characters is worthy of very careful consideration. He claims that thirteen out of the twenty-three Phœnician characters have equally done duty as graphic signs on the Asylienne pebbles, and says: "This is a considerable proportion, especially as the number of the Phœnician letters would at first have been fewer, and therefore the idea that all letters have been borrowed entirely from Egyptian writing falls to the ground. The Phœnicians, a mercantile people, whose navies frequented all the Mediterranean shores, have everywhere taken with them, and notably to the neighbourhood of the Pyrenees, the graphic signs which appeared to them the most convenient for keeping their registers, for dealing, and for correspondence. They selected the most widely distributed signs in the countries where they trafficked, in order to spread more rapidly the innovations which they introduced into writing. They changed the meaning of the ancient signs. Those of Mas-d'Azil were probably originally syllabic. In becoming letters, their significance was necessarily modified; but their form was not altered. The Greeks, whose ancient syllabaries had many characters in common with the Asylienne alphabet, adopted the Phœnician

reform all the more easily, since they found in the Phœnician letters forms with which they were familiar; later, by a very natural reaction, their tendency was to restore to the signs, which the Phœnicians had modified, their ancient configuration, which they still remembered."

The number of the signs which Mr. Piette believes to be syllabic is small compared with those with bands and spots, and an obvious objection to his view is that, with very few exceptions, each syllable is on a different stone; in M we reproduce the figure of one pebble which has two syllabic signs printed on its side. Have we here the traces of the oldest known seat of learning, which succeeded a still earlier art school, and at which the early Neolithic inhabitants of the Pyrenees learnt how to count and how to build up words from syllabic signs? It is tempting to follow up this line of speculation, but we do not wish to diminish the seriousness of Mr. Piette's labours by unintentional levity.

Specialists alone can determine the real value of Mr. Piette's discovery, but it is evident that he has opened a new chapter in the early history of man, which promises to be of exceeding interest, and we venture to offer him our sincere congratulations.

The paper is illustrated by 107 figures in the text, and it is accompanied by an atlas of 25 plates, containing some hundreds of figures, which are very beautifully printed in colours.

A. C. HADDON.

IV.

Cope's "Factors of Evolution."[1]

THE object of this book is "to select from the mass of facts accumulated by biologists, those which, in the author's opinion, throw a clear light on the problem of organic evolution, and especially that of the animal kingdom." The selection, however, is chiefly made from the facts of palæontology, with a view to interweaving them with the evidence from bionomics and embryology that has been set forth by others. The author poses not as a judge, but as an advocate, and the evidence that he marshals is intended to prove the following propositions: (1) Variations appear in definite directions; (2) Variations are caused by the interaction of the organism and its environment; (3) Acquired variations (which I shall call modifications) are inherited; (4) Variations survive directly as they are adapted to changing environments; (5) Movements of the organism are directed by sensation and other conscious states; (6) Habitual movements are derived from conscious experience; (7) The rational mind is developed by experience, through memory and classification.

Natural selection, then, is admitted by thesis 4, but, according to the other theses, is left little work to do. For, if variation be definite, if it, whether as congenital variation or as inheritable modification, be induced by change of environment, or be, as theses 5 and 6 seem to imply, a more or less conscious response thereto—then, if this method of evolution be a workable method at all, it is a self-sufficient method. Natural selection, if these six theses be true, will find no variations out of harmony with the environment, and will have to seek a situation in another universe. But one can admit the truth of all these propositions without admitting it to be the whole truth. There might still be variations that were indefinite and not in directions determined by environment, modifications that were not inherited, and movements that were not the result of consciousness in any accepted sense of the word. The march of events is not bound to be on any simple system dreamt of in our small philosophies. In short the Neo-Darwinian is not compelled to cut the throat of the Neo-Lamarckian, but may join the agnostics, of whom there are still a

[1] The Primary Factors of Organic Evolution. By E. D. Cope. 8vo. Pp. xiv., 548, and 121 text-figures. Chicago and London: The Open Court Publishing Co. 1896. Price 2 dols.

few, in discussing the arguments here ably presented by Professor Cope.

First, the author attempts to show that variations are definite and in determined directions. That there are limits of variation in each species may be admitted, whether the limits be wide or narrow, whether they be due to obvious physical and chemical conditions or to characters impressed on the race by ancestral history. That allied species, or the varieties of a species, and especially geographical subspecies, observe a successional relation to each other, is a fact that no one need hesitate to accept after reading the numerous instances here brought forward. A lizard, to select but one example, does not jump from a longitudinally striped to a transversely banded form, but the two are connected by intermediate series of broken stripes, spots, and broken bands, a circumstance tending to show that one has been derived from the other; and when we enquire which it is that has been derived, we cannot ignore the further fact that forms banded in the adult have young that are striped and pass through a spotted stage. Nor is this relation confined to superficial characters; it appears no less plainly in the teeth of mammals, in the shoulder-girdles of frogs, and in the arm-loops of brachiopods. We accept all this; but we accept it as a statement of the trend of evolution, not as expressing the direction of variation. With the exception of a paragraph quoted (without reference) from a paper in NATURAL SCIENCE by the Rev. G. Henslow, there is no attempt to show that all the offspring of any individual vary, if they vary, in one direction, and that the direction is constant for all individuals of a species. Indeed, it is hard to see why the main facts detailed by Professor Cope in this chapter cannot be explained just as well on a hypothesis of natural selection, although, in the enforced absence of evidence, the explanation would be worth just as little.

The next chapter deals with phylogeny, especially with various lines of descent within the Vertebrata, which, by reason of definite palæontological evidence, are better known than any others. The position of these facts in Professor Cope's argument appears to be stated in this paragraph:—"Examination of all these genealogical lines reveals a certain definiteness of end and directness of approach. We discover no accessions of character which are afterwards lost, as would naturally occur as a result of undirected variation. Nor do we discover anything like the appearance of sports along the line, the word sport being used in the sense of a variation widely divergent from its immediate ancestor. On the contrary, the more thorough becomes our knowledge of the series, the more evident does it become that progressive evolution has advanced by minute increments along a definite line, and that variations off this line have not exerted an appreciable influence on the result." This statement, just as the preceding ones, may be accepted; but, equally with them, its bearing on the question is not obvious. In saying that there are no accessions

of characters afterwards lost, Professor Cope cannot mean to deny that there have been lines of progressive evolution (*e.g.* the Trimerellidæ among Brachiopoda and the Calceocrinidæ among Echinoderma), which rapidly became extinct; nor can he mean to deny that, in the history of a line, a character (such as the coiling of Ammonoidea) has gradually appeared and, as gradually, disappeared. What his examples show him to mean is, that within the limits of any one ascending series, there is no appearance of a character outside the general trend of development. The inception and evolution of characters are gradual processes, and a line of development once started is continued. What is this, other than to say that variations are so slight as to be imperceptible to our grosser sense; that those not in accord with environment are checked before they make their mark on the history of the race; that those in accord with environment are preserved; and, since environment changes gradually and definitely, that preserved variations and the consequent line of organic evolution are also definite and gradual? So far there seems nothing in the statements of our author that may not be accepted with untroubled conscience by the most orthodox of Neo-Darwinians.

The final section of the same chapter can offend none except those who would derive the Vertebrata from a highly specialised arthropod, for it merely restates the fact that "the highly developed, or specialised types of one geologic period have not been the parents of the types of succeeding periods, but that the descent has been derived from the less specialised of preceding ages." This, however, does not even profess to bear on the main argument.

Chapter III. is devoted to exemplifying what Professor Cope calls the Canon of Parallelism. This, put as broadly as possible, and without the qualifications that are required for each particular case, states that "all organisms in their embryonic and later growth pass through stages which recapitulate the successive permanent conditions of their ancestry. Hence those which traverse fewer stages resemble or are *parallel* with the young of those which traverse more numerous stages." As an instance of this may be mentioned the case of the lizard cited above; but Professor Cope has had no difficulty in adducing plenty of cases from various branches of the animal kingdom. He further explains how inexact parallelism (cænogenesis of Haeckel) may arise through certain characters developing more or less rapidly in certain lines of descent than they do in others. Inexact parallelism is, of course, the rule, while the necessity for compression in development, or the opposing influence of an altered environment, has often caused the partial or almost complete elimination from ontogeny of recapitulatory stages. Accepting this most freely, still one cannot shut one's eyes to the facts of parallelism, in many cases so obvious; and despite the amount of criticism that the principle has met with of late, it really seems as though the only differences between the various writers lay in their interpretation of admitted facts. What bearing he

supposes the facts to have upon any of his theses, Professor Cope does not, so far as I can discover, deign to tell us; nor is it clear how the facts of successional relation in time are more valuable evidence than those of successional relation in space, which were dealt with in the first chapter.

The facts of retrogressive evolution or degeneration, which Professor Cope calls Catagenesis, are discussed in Chapter IV., and are supposed to prove the direct influence of disuse; but no attempt is made to combat the arguments of many who believe themselves to have shown that degeneration is merely a form of adaptation to surroundings, perfectly explicable on the principles of natural selection.

At this stage Professor Cope points out the conclusions he thinks he has reached. For my part, while prepared to admit that "phylogeny exhibits a progressive advance along certain main lines," and that evolution itself has been in definite directions, I cannot see that it has been proved that all or even the majority of variations were in definite directions. Definite variation may be possible, may exist, may be of constant occurrence; but as an attempt to prove it, these 200 pages seem to me utterly beside the point.

We come now to the second part of the book, and as this is entitled "The Causes of Variation," we look for something in support of the thesis that variations are caused by the interaction of the organism and its environment. Instead of this we find a lengthy and highly interesting account of various methods in which modifications are produced by the direct action of external influences, among which are included the mechanical actions of the parts of the organism itself. Everyone may not agree with everything here brought forward, but none can be prepared to deny all the instances. Let us, for the sake of the argument, admit modifications of the adult or of the growing organism to the extent that Professor Cope desires. The question remains: are these modifications inherited? Throughout this section such inheritance is boldly assumed by the author, and so all difficulties disappear. But the mere existence of modifications, or the occurrence of evolutionary series, the goal of which appears to be not merely definite but adaptive, of themselves prove nothing. Undoubtedly, if one could prove modifications to be inherited, one would have a simple and satisfactory explanation of a very large number of facts which at present have to be explained as due to some exceedingly tortuous process, and one that many straightforward people find difficult of conception. But we must remember how unsafe it is to assume that simplicity is the rule in nature.

Palæontologists, as a rule,—and Professor Cope is one of them—are so profoundly impressed by the adaptive nature of the evolutionary process and by the definiteness of its direction, that they cannot regard the restraining or selective action of the environment as enough to keep the breed true. They are so accustomed to see mutation

after mutation, generation after generation, developing in apparent obedience to obvious physico-chemical or mechanical conditions, that they incline to regard these conditions as causes. And if it be suggested to them that the results they see may have been achieved by the selection of adaptive variations from among a number of promiscuous variations that were not adaptive, they ask why it is that they do not find evidence of these numerous non-adaptive variations in the rocks, when one would suppose that, on any hypothesis except that of definite variation, such forms must have been the more abundant of the two. It is useless to reply to them that the non-adaptive variations in each generation were killed off when young, and so, even if fossilised, are practically undistinguishable; because they will reply with abundant proof that the adaptive characters chiefly appear in the adult stages of the organism, possibly only in its senile stages, and so are incapable of coming under the action of natural selection during the early undifferentiated stages. How the conversation might continue does not much matter, for it is obvious that it has reached a point beyond which all must be speculation. The facts on which the palæontologist relies, the facts that Professor Cope adduces with such wealth of knowledge, are strong presumptive evidence in favour of his second thesis. But they are not proof.

"Chapter VII.—Natural Selection" is included in this part, not because the author regards natural selection as a cause of variation, but because he wishes to controvert that view. It is often stated in the writings of those who are not strict Neo-Darwinians, that some, at least, of the Neo-Darwinians regard natural selection as a cause of variation. If this be so, Professor Cope is surely right in affirming such Neo-Darwinians to be in error. "A selection cannot be the cause of those alternatives from which it selects." To state that natural selection may preserve the character of variability is another matter; for variability is just as much a character of some organisms as fixity is a character of others, and this without reference to the environment; and since variability is a character that may work good or ill to an organism, it inevitably falls under the action of selection. Apart from this, is there not a slight misunderstanding here, due perhaps to a lax use of the term "natural selection?" Whereas "selection" means selection and no more, the stereotyped phrase "natural selection" has come to imply the whole process that forms the fundamental conception of Darwinism. For evolution to take place through this process, we must postulate:—first, variation in the offspring; secondly, variation in the environment; thirdly, selection of such variations in the offspring as are in harmony with the variation in the environment. It is this whole series that results in change—change which is definite in kind, and which influences many individuals in the same way. In short, this is "the origin of species by natural selection."

However elementary the above exposition may seem to some, it

seems to be needed by Professor Cope. For he says (p. 7) "Mr. Spencer occasionally falls into the error of ascribing the origin of structures to natural selection, as in the case of the forms of flowers (Principles of Biology, II, p. 153), and the armor-plates of paleozoic fishes (*Op. cit. p.* 288)." What Mr. Spencer does say is (on p. 153), "if we bear in mind the functions of flowers, we shall find in their adaptations to their functions, under conditions that are extremely varied, an adequate cause for the different types of symmetry, &c.," and again, "the forms and positions of those subsidiary parts which give the general shape to the flower, similarly arise by the survival of individuals which have the subsidiary parts so adjusted as to aid this fertilizing process." And what Mr. Spencer says on p. 288 is, "The contrasts between . . . . soft skin Fish and Fish in armour like the *Pterichthys*, must have been produced entirely by natural selection." Moreover, on page 192 Professor Cope says that Wallace ascribes the colour and form characters of animals "to natural selection *as a cause*." This time no reference is given, so one can merely draw attention to the fact that nothing in Mr. Wallace's book "Darwinism" supports this statement; on the contrary, its author continually postulates the existence of variation before selection, he accepts sexual reproduction as the main, if not the only, cause of variation, and, as for colour, he regards it "as a normal product of organisation, which has either been allowed free play, or has been checked and modified for the benefit of the species" (p. 299).

From the point of view of the argument, the third part of the book, entitled "The Inheritance of Variation," is by far the most important; for by "variation" Professor Cope usually means what is here called "modification." If the inheritance of modification be proved in a single instance, then it is a possibility, and as such may legitimately be drawn upon in constructing a theory of organic evolution. Is it proved here? The first argument is that certain characters were, at some time or other in the history of each line of descent, acquired, *i.e.*, they first appeared as modifications of adult structure; and that these characters are no longer induced afresh in each generation, but are inherited. In short, "*all characters now congenital have been at some period or another acquired.*" For instance, the groove-joints between the metapodial bones and the phalanges of the mammalian foot are assumed to have been due originally to mechanical causes; they are no longer so due, since they are to be found prefigured in cartilage in young animals that have never stood on their feet. The Neo-Darwinian sees no difficulty here: first, he simply denies that such characters were originally produced by mechanical causes; secondly, he maintains that, even if they were so produced, yet the inherited characters were really congenital variations that happened to be in the same direction as the modifications. It is clear that such an argument cannot be disproved, so we pass, with our author, to the evidence from palæontology. Here Professor Hyatt entirely fills the

stage with his case of the impressed zone of the Nautiloidea, of which an account was given in NATURAL SCIENCE, vol. III, p. 408. "There is" says this palæontologist "every reason for regarding the impressed zone as a ctetic [*i.e.* acquired] characteristic acquired in the later stages of growth and not hereditary so far as is known in any shells of the earlier Paleozoic periods." Admitting this, one still remains without definite proof, and it is unfortunate that even Professor Hyatt, in a subsequent note, has to admit certain exceptions, of which "it is impossible to say at present whether the impressed zone appeared as a genetic character or as a mechanical necessity." Here again the evidence, however cumulative, will receive from the Neo-Darwinian the same explanation as the evidence from embryology.

The evidence from breeding is quoted chiefly from Professor W. H. Brewer, and puts the case very strongly. It is pointed out that "all the best breeders recognise the rule laid down by Darwin, that those characters are transmitted with most persistency which have been handed down through the longest line of ancestry." Hence, they do not expect modifications produced by temporary conditions, or by conditions that have been in operation for only a short period, to be inherited. On the other hand, says Brewer, "The art of breeding has become in a measure an applied science; the enormous economic interests involved stimulate observation and study, and what is the practical result? This ten years of active promulgation of the new theory has not resulted in the conversion of a single known breeder to the extent of inducing him to conform his methods and practice to the theory. My conclusion is that they are essentially right in their deductions founded on their experience and observations, namely, that acquired characters may be, and sometimes are, transmitted." The cases of inheritance of injuries cited by Brewer appear well authenticated, but what is the use of them when they can so easily be explained away, by Neo-Darwinians, as mere coincidences? A case more akin to what would, on the hypothesis of the inheritance of modifications, be the action of nature, is the following: Sheep taken from Ohio, where the wool is fine and good, to the alkaline soil of Texas, have their wool rendered harsh in texture, while its behaviour under dyes is altered. So far this is a case of undoubted modification due to environment. The offspring are born with wool of the same harsh character, and this alone might be explained as a modification induced afresh in each generation. But it is noticed that the harshness increases with succeeding generations, and flocks that have inhabited such regions for several generations produce a harsher wool than do the new-comers. It thus appears that there is something more than a mere repetition of the modification, and this something more is explained most naturally as due to inheritance of the modification. To explain it as "a congenital adventitious variation coincident in all the individuals of immense flocks, is a mathematical absurdity." The only possible alternative appears to

be that the environment, which *ex hypothesi* is favourable to individuals with harsher wool, selects such individuals from among the numerous lambs, some of which may of course vary in the direction of harsher wool, while others vary away from it. But now comes the final step in the Lamarckian's argument, which establishes him so firmly that it is hard to see how the Neo-Darwinian can dislodge him from his position. It is this: so far from environment having any selective action, the selection is all the other way, since the breeders do all they can to lessen the evil by selecting the lambs that vary least in the direction of harsher wool. Now, if these facts are really so (and we have them presented to us with others of similar nature under the highest authority), then the only conclusions I am able to draw are, first, that environment can modify individuals; second, that variation can be so influenced by environment as to be definite in direction; third, that modification can be transmitted from parent to offspring; fourth, that not merely individuals but the whole line of descent may respond to the direct action of environment in the face of equally direct and opposing action on the part of selection. In the case of these sheep, it is not definitely stated, doubtless because it is a well-recognised fact in this as in other cases cited, that the offspring of such animals, though born and reared in favourable localities, do not, in the first generation at all events, throw off their unfavourable characters. This is further proof that a definite effect has been produced, not merely on the individual but on the race, by the direct action of environment.

Professor Cope has been a long time in giving us this proof. It seems as though palæontological evidence alone will never be enough to furnish any of equal value, and that we must in the end look to the experimenter. One cannot have proof unless one has control of the conditions of experiment; mere historical results are capable of so many interpretations. To apportion guilt and innocence justly between Queen Elizabeth and Mary Queen of Scots is child's play compared with the task of deciding how much of the structure of an ammonite is due to congenital variation selected by environment, and how much is due to modification impressed by environment.

Yet let not Professor Cope lay to his soul the flattering unction that this case of the sheep, or any other that he quotes, will satisfy the Neo-Darwinians. Every Neo-Darwinian must be his own experimenter, and as few of them have the capital to invest in sheep-runs in Ohio and Texas, they will probably prefer the easy laziness of scepticism. And even should one of them verify this instance to his own satisfaction, he will at the same time have invented some new explanation, or some sweet arrangement in iddities, that will leave him as unconvinced as ever.

Let us, however, who are neither Neo-Darwinians nor Neo-Lamarckians, admit that Professor Cope has scored his first four points, if only that we may see the game out. Every explanation of

the facts around us necessitates another explanation behind it, and so, if modifications are inherited, the biologist has to explain how they are inherited. This is why Weismann appealed to us: not because he explained evolution, for Darwin, Wallace, and Spencer had done that, but because he explained their explanation, showing how it worked through the known mechanics of reproduction and cell-division. Now the explanation of evolution by Lamarck is not opposed to the explanation by Darwin, it is complementary to it. What it is opposed to is the germ-plasm theory, at all events as first strictly stated by Weismann. Professor Cope, therefore, who follows Lamarck, is obliged to provide some explanation of the way in which a modification of the adult soma is transmitted to the already differentiated germ-plasm. His first step is to propound what he calls the Theory of Diplogenesis. This supposes that a cause affecting the soma actually, likewise affects the germ-plasm, but affects it potentially. It is hard for us to understand how an external cause influences the soma, but we know that it does so; it is hard for us to understand how the germ grows into the adult with the characters of the parent, but we know that it does so; is it any harder for us to understand how the same external cause may influence the germ while yet within the parent?

Professor Cope, it is true, does not present us with a wonderful mechanism of speculative *ids*, *idants*, and the rest, linked to a set of physical appearances upon which no two cytologists are agreed, and so we must admit that his explanation, even if it be accepted, requires some further explanation. But, so far as this goes, his theory is no worse than anyone else's, and it seems safer to wait for the principles of cellular biology to settle before applying them to these speculative questions. Rejecting all hypotheses based on pangenetic principles, *i.e.* on the transference of material character-bearing particles, Professor Cope relies on the action of a growth-force, to which he gives the name "bathmism." This in its turn requires explanation, and it is explained as the action of a kind of unconscious memory, with which faculty the reproductive cells are endued. Impressions are transmitted to these cells through the continuous protoplasm of the organism, and the cells retain, "first the impressions received during their primitive unicellular ancestral condition, and second, those which they have acquired through the organism of which they have been and are only a part." When we are able to give a physical explanation of conscious memory, it will be time enough to consider this heredity theory of Professor Cope, or rather of Hering. At present, like many other fascinating speculations, it is as incapable of proof as of disproof.

The final section of Chapter VIII. recapitulates some of the objections that have been raised to the doctrine of the inheritance of modifications. This, however, chiefly discusses special cases, with which the theory neither stands nor falls.

Chapter IX., headed "The Energy of Evolution," appears to be devoted rather to a recapitulation of the theory of bathmism, to the explanation of heredity as the transmission of a mode of motion, not the transmission of matter, and to the airing of those numerous neologisms in which the New World delights—Antichemism, Emphytism, Statogenesis, Autokinetogenesis, Cryptonoÿ, and the like. It is Chapter X. that tells us what, in Professor Cope's opinion, does form the energy of evolution. "It maintains that consciousness as well as life preceded organism, and has been the *primum mobile* in the creation of organic structure. . . . The true definition of life is, *energy directed by sensibility, or by a mechanism which has originated under the direction of sensibility*." To enter upon the discussion of such important theses would require not a paragraph, not an article, but a book. It is the less necessary since the views of Professor Cope have recently been fully criticised by professed philosophers and psychologists (see NATURAL SCIENCE, ix., p. 220).

Chapter XI., and last, contains a list of papers by American authors that have contributed to the evidence used in the book, and summarises in their own words the positions assumed by Ryder, Hyatt, Packard, Osborn, Dall, W. B. Scott, Eimer, and Naegeli. Whatever our own opinions may be on particular questions, we must admit that this chapter, and indeed the whole book, forms a useful guide to the views of the American school of biologists, and especially to those of its chief prophet, Professor Cope. At the same time we must remember that not all eminent biologists in America are Neo-Lamarckians, any more than all in England are Neo-Darwinians. In biology, as in other sciences, there is still plenty of room for that diversity of opinion without which knowledge cannot be advanced.

F. A. BATHER.

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V.

Wasps and Weismann.

THE phenomena connected with the reproduction of the social Hymenoptera are of such interest, not only for their own sake but also on general considerations, that the results detailed by Dr. Paul Marchal in a paper entitled "La Réproduction et l'Évolution des Guêpes Sociales" (*Arch. Zool. Expér. et Gén.*, 3e sér. vol. iv. pp. 1–100, 1896), deserve more than cursory notice. His experiments deal with various species of wasps; and in order to give a fair presentation of his theoretical conclusions, the experimental results must be briefly stated.

In terrestrial species the size of the workers increases progressively from the beginning to the end of the season, culminating in intermediate forms not much smaller than the queens, which are the final product of any one complete season. Correspondingly, the lower and later combs are formed of larger cells for the reception of queen larvæ, though of these a few may at first contain large "intermediate workers"; while the small cells contain at first workers only, and later both workers and drones, the latter becoming more numerous as the season advances. Contrary to the statement of Réaumur, no special cells are constructed for the reception of the male (drone) larvæ. Combs containing both large and small cells occur at times, and in such the proportion of males is unusually low. The normal occurrence of fertile parthenogenetic workers producing male offspring only is beyond doubt. Such workers become extremely abundant if the queen is removed, or if her fecundity becomes impaired; but apparently only recently emerged imagines can attain to this distinction. The determining cause of this occurrence is the excess of food consumed by the workers, in consequence of the absence of larvæ, to whom normally they would impart a large share of the food-material brought to the nest. It is fully established that quantity and not quality of food is the determining factor in the rearing of worker or queen wasps.

The power of the queen to withhold spermatozoa from those eggs destined to be males, and with certainty to fertilise those deposited in queen cells, has been ascribed to the will. Dr. Marchal suggests a new explanation as follows:—towards the end of the season the spermatheca contracts with less regularity for various reasons, hence

many unfertilised ova are deposited: the larger cells excite the queen and furnish a stimulus which causes the spermatheca to contract and expel spermatozoa, so that all large cells contain fertilised ova, of which the great majority give rise to queens. The explanation is ingenious, but does not appear to reach the bottom of the matter. It is probable that the explanation of the phenomena is the same for both bees and wasps, and, if so, the question arises,—how is it that the large drone-cells of bees do not act in the same, but in the contrary manner, on the nervous system of the queen bee? It is tempting to attribute the sure fertilisation of ova towards the end of the season to the elongation of the abdomen of the queen wasp in order to reach the bottom, or rather top, of the large cells; such elongation would cause a narrowing of the abdomen and put some pressure on the walls of the spermatheca. A similar view has been held with regard to the queen bee, but this supposition is effectually disposed of by the observations of Drory and others, that the queen bee will lay drone-ova in small cells if all large are removed, and, conversely, fertilised eggs in large cells if provided only with the latter. Incidentally it may be mentioned that Dr. Marchal finds that the bifurcation of the seminal duct as described by Cheshire is non-existent.

The arboreal species of wasps do not form special cells for the queen larvæ, but the existing small ones are increased in height, and, in consequence of their shape, also in diameter.

An evolutionary series in nest economy is thus presented, commencing with *Polistes*, and passing, by the arboreal and then the terrestrial wasps, to the honey-bee.

An observation of great interest consists in the establishment by experiment of the inability, despite desire, of male *Vespa germanica* to copulate with female *V. vulgaris*, or vice versâ. This fact at once demonstrates the importance of the male genital armature in determining species.

En passant the immunity of the parasitic fly *Volucella zonaria* is attributed to the quiet slow way in which it moves about within the nest, in striking contrast to the behaviour of a strange wasp.

After stating results and conclusions, Dr. Marchal proceeds to an examination of Weismann's theory, and an explanation of the phenomena of heredity here presented. Determinants, ids, and idants are rejected as having no real existence. The sterility of the early workers is regarded as being due (1) to insufficient food supplied by the over-prolific queen, (2) to these workers having to supply food to their very numerous brethren. With the advance of the season, and increase in numbers of active workers, the latter become capable of coping with their cares, and thus acquire more food for themselves, while each larva also gets greater abundance; thus there arises a series of larger workers, some of which are fertile, and eventually queens appear. Proceeding now to the effect on the germ-plasm, it is maintained that the present conditions of rearing at the end of the

season differ from the variable conditions which obtained in pre-social ancestors; that these newer conditions constitute a constant modifying influence on the germ-plasm, which has thus acquired a new physico-chemical constitution. The ova that give rise to workers contain this same modified germ-plasm, but the resulting larvæ are reared under conditions different from those which have now, in the course of ages, become normal to the queens. The germ-plasm is therefore under new conditions, and does not follow its normal course, but gives rise to imperfectly developed forms (workers). As the conditions more and more nearly approach those habitual to queen-ova and queen-larvæ, so does the resulting imago more and more nearly attain the perfect form. All cases of dimorphism and polymorphism among social insects with sterile forms are capable of being similarly explained. These conditions, once reached, are maintained by natural selection.

In a second paper, "Observations sur les Polistes" (*Bull. Soc. Zool. France*, vol. xxi., pp. 15–21, 1896), Dr. Marchal points out the habit of *Polistes* of storing small quantities of honey temporarily; also that the hexagonal form of the cell is not due to instinct, but to compression, the first part of the cell being cylindrical.

Dr. Marchal is to be congratulated on having brought together by his own industry a number of valuable observations, and on furnishing a sound explanation of the main problem before him.

OSWALD H. LATTER.

Charterhouse, Godalming.

SOME NEW BOOKS.

Our Forgotten Forefathers.

Prehistoric Man and Beast. By Rev. H. N. Hutchinson. 8vo. Pp. xxii., and 298, with 10 plates. London: Smith, Elder, & Co., 1896. Price 10s. 6d.

This is a fairly good attempt to collect and summarize what has been published in books and memoirs (for the most part English) about Prehistoric Man, with illustrations of his probable ways of life, taken from the nature and customs of existing savages. It is comprehensive and plainly expressed; indeed, the author, intending to write in a popular style, condescends to use a slang word here and there.

The gradual outcome of creatures and things, not only the manifold inhabitants of the earth from earliest times to the present, but the structure and conditions of our globe, and the solar system itself, is one of the leading ideas throughout this well-wrought compilation of facts and notions about "Prehistoric Man and Beast."

Part I. comprises seven chapters treating of "The Man of the Older Stone Age" (Palæolithic). The stone implements are briefly described at page 28, and do not appear to have been specially studied by the author. He notes some of the geological deposits, chiefly in valleys and caves, in which these stone tools and weapons have been found. The "Plateau Implements" of Kent, and the gravel in which they lie, are better known now than when page 24 was written; and their value is not really influenced by the depreciative doubts of some referred to. The prehistoric *human remains* are carefully noticed, especially those found in Belgium.

The author expresses a strong distrust in the published evidence of Pliocene or Miocene Man. He might as well have been cautious in this matter. Dr. Noetling particularly refers the Burmese flint flakes (see Natural Science, vol. v., p. 345) to a ferruginous conglomerate of early Pliocene Age, and he writes that he did find them in the real conglomerate, notwithstanding what the author's "Old Schoolfellow" may state to the contrary. Dr. Hugh (why "Keith"?) Falconer's expectation that man or his works will be found in the Sivalik strata, or their equivalents, may still be well grounded.

Mr. W. G. Smith's interesting book on "Man the Primeval Savage," his implements and ways of life, is largely drawn upon; and like that laudable attempt to describe and illustrate prehistoric man and his surroundings, the book before us is calculated to raise and encourage a taste for this subject, putting the enquirer in the right line of research, and affording many clues to first-class systematic works on anthropology.

The rock-shelters and caves of Dordogne and neighbouring districts in France form the subject of chapter iii., taken from good sources. Their former occupants are known as "Reindeer Hunters." Some interesting notes taken by travellers and others about existing savages and their modes of living and thinking are brought in as appropriate to the probable condition of these

prehistoric Aquitanians and others. At p. 69 is a lively description of plate iii. which represents two men trying to follow up some reindeer, one of which has been already wounded with a harpoon, probably when first met with while grazing. A far easier method of obtaining their venison, by the old folk, was probably by trapping in pitfalls and the associated run-ways and deer-fences, as in Scandinavia and Newfoundland. At p. 70 the curious pogamogans used in those old times in Western Europe, and by some wild races at the present day, are mentioned. We may inform the author that in Westminster Abbey a pogamogan is held in the hand of a North-American Indian, who has been sculptured as part of a tombstone in memory of some military officer in George the Third's day. It is on the north wall of the nave.

Information collected about ancient cave-dwellers, especially the men of the Older Stone Age, opens out the subject of rock-shelters, caverns, and especially bone-caves. In the last occur bones of animals formerly existing, but some of them now extinct altogether or removed by emigration from their former country—hence the question as to the time when the caves were occupied, and, of course, the age of the bones in the several caves, in relation especially to the so-called Glacial Period. These points are more or less dogmatically stated by different observers. It is often difficult to determine whether such animals as now inhabit warm climates were really associated at the same time with such as are now accustomed to live in cold regions. Also whether or no, clay and stones rubbed down by then existing glaciers entered or shut up the mouths of the said caves. These questions often cannot be decided for want of incontestable evidence.

The various proofs that Western Europe had a kind of Arctic climate, in palæolithic and probably neolithic ages, lead to the consideration of its possible causes. Although some extreme statements and fancies have been cleared away, the real cause or causes are still hypothetical. This is the subject of chapter iv., "On the Myth of the Great Ice-sheet, and Theories of the Flood." Mr. Hutchinson wisely regards the "Ice-age" as having been greatly exaggerated—that "the notion of a Polar Ice-cap must be for ever abandoned," and that "vast physical changes" took place at the end of the Glacial Period, with an "amelioration of the climate." He finds no room to compile an account of the peculiar Drift deposits of the time; but objects to Prestwich's careful and elaborate geological exposition of "a rapid submergence and emergence," producing all the apparent results of a deluge in Western and Southern Europe; also to his own friend Howorth's "wide-spread wave of waters" passing over the country, and sweeping away the mammoth and its contemporaries. This flood was presumably caused by earth-movements, as the foregoing was, but on a larger scale. The Reverend Author rejects these scientific hypotheses, because "both the account in 'Genesis' and that in the primitive Accadian or Chaldean version mention *rain* as the cause of the Flood"!

Changes in climate and their causes, in chapter v., form a sequel to the "Great Ice-sheet Myth"; and some enquirers may find the *resumé* of opinions and calculations of use to them in their studies. As for the author, he is judiciously cautious here; the old-fashioned idea of the relative positions of land and water—that is, geographical conditions and their changes—is mentioned; and the cause of the Glacial Period is left "an open question." Less than 20,000 years is taken as being fully the time between it and the present. So also in

chapter vi., on "The Antiquity of Man," the recession of Niagara is known to give correlative time of 12,000 to 18,450 years since the Glacial Period. Other standards for the calculation of this time are quoted at p. 139.

In chapter ix., the abodes of the living and sepulchres of the dead of these prehistoric times are described. The kitchen-middens and shell-mounds are useful indications of modes and conditions of life in many parts of the world. So are the mounds, barrows, tumuli, and other tombs, of the character of the deceased, and of the culture of the survivors. Much interesting detail is collected from writers on these and collateral subjects.

The former existence of a short-statured race throughout Europe and the British Isles is strongly believed by Mr. Hutchinson. An early expression thereof occurs at p. 30, where he seems to suppose that small flint-flakes must have belonged to a small kind of men. He mentions "Moustier in the Dordogne district" as connected somehow with evidence of this. He gives no reference thereto.

Chapter x., for its main subject, has the "Little-folk or Fairies, and Mermen." The information has been collected *con amore*. The stone arrows of the Dwarfs seem to be the connecting link with the neolithic times just now alluded to. Boyd Dawkins' descriptive sketch of a neolithic homestead, and Flinders Petrie's neolithic intruders into Egypt about 5,000 years ago also find a place in this chapter.

In the two following chapters the author gives fuller remarks on the Dwarfs, Little-folk, Troglodytes, Dwellers in caves and underground, Goblins, Elves, and Fairies, all of whom originally, as well as the Mermen, he regards as having belonged to the race of the Finns, Lapps, and Esquimaux. To these Fairies or Dwarfs he ascribes the building of Stonehenge.

In connection with the indications of the important changes from the use of implements of wood and stone to those of bronze and iron, we would have liked to have seen our old acquaintance, cunning little Jack the Giant-killer, brought in with his metal sword, superior to the clubs of bigger and stupid men, although his history is far from being equivalent to the clever stone-using Dwarfs fighting and annoying, if not beating, men of metal.

Regarding the antiquity of the human race, it is thought "very probable that man originated at a somewhat early period in the great Tertiary era of the world's history" (p. 128); but, being under the the rule of evolution, and having his immediate ancestors among the Primates, the geological horizon of those ancestors must be taken as a limit before which he cannot have come upon the stage. The gradational characters of the skulls from Spy, Neanderthal, Naulette, etc., and the still lower type of the skull from Java, prepare us for the "missing link," whether coming from Pleistocene, Pliocene, or even Miocene strata. We have already intimated that Mr. Oldham's attack on the accuracy of Dr. Noetling's observations has not demolished the Burmese evidence; and the Javan skull still retains its semi-human, or even infra-human, status according to several experts.

In Part II., Mr. Hutchinson treats of Man of the Later Stone Age (Neolithic), and the Bronze Age; and herein he moves freely among the published discoveries and descriptions of Lake-villages, Pile-dwellings, and Crannogs, and the implements and other curiosities found in their remains; as well as in the tombs and burials of the same kind of peoples. Some pile-structures in London (Pitt-Rivers)

and in Newbury (E. P. Richards) might have been mentioned for England.

In chapter xi., on "Rude Stone Monuments," Mr. Hutchinson has collected, from Fergusson and others, various notes about Cromlechs, Dolmens, Pillar-stones, Monoliths, Standing Stones, and Menhirs, also about lines and avenues of stones—these are well-known evidences of the work of some prehistoric peoples. Not knowing anything more than others about the feelings and intentions of the original makers of these rough stone-works and earth-works, the author uses a judicious caution for the most part. But he apparently gets out of temper with those who have called Stone-circles Temples (p. 265); and especially with those who have supposed the *Druids* to have been connected with their origin or uses; nor may the Keltic people be hypothetically connected therewith. Stonehenge is a sad trouble to our author; he knows apparently, like others, just what he has read about it, after or before an occasional inspection of the place. After suggesting that Pre-keltic Dwarfs may have built it, because they were clever at "chambered cairns and dolmens," he finishes with eighteen observations and arguments amounting to little, except that "Stonehenge may be purely memorial," but is not merely a burial place; he thinks that it has not any astronomical or solar meaning, but that it may have been used for public meetings and a court of justice. We cannot here indulge in any controversial remarks for the sake of helping the author or his readers, although there are many points of interest tempting to the philologist and archæologist. A new course of reading about Stonehenge, even in the writings quoted by himself, might improve the second edition to which this book is well entitled.

In a new edition the very interesting and suggestive researches of Dr. J. S. Phené and others on shaped mounds, having definite outlines of animals, with associated stone-work of probably sepulchral and sacred meanings, in different parts of the world, should be fully referred to.

The plates should be inscribed with the pages where they are described; the list of publications should have some kind of classification, or (best) a chronological arrangement; and there should be a better Index.

In the later chapters, as indeed with other parts of the volume, the author has conscientiously tried to fulfil his intention "to gather and put the scattered threads of inquiry into a continuous web and pattern" (page vii.), so that every-day readers may know something definite (if not conclusive) about the too frequently imperfect and confused statements and notions concerning Man in his early existence on the Earth.

The ten phototint plates, very suggestive of the author's views, have been carefully designed by Mr. Cecil Aldin, but the original drawings, of larger size, lately exhibited at the Geological Society's *soirée*, had a more artistic aspect.

Plate I., "An eviction scene at Wookey Hole, near Wells, Older Stone Age," is described at page 53, as the defence (of very doubtful success) of his cave, woman, and child, by a skin-clad man, against a select lot of the carnivorous beasts of the period.

Plate II., "Hunting the Mammoth in Southern France," shows one hairy elephant marching along with apparent disregard of javelins and stones, and another coming up behind with a man lifted up in his coiled trunk. The picture might be termed elephants hunting men. Possibly in front of the first elephant is a part of a badly

constructed cover of a pitfall, and he has taken up a twig or two by his trunk.

Plate III., "Hunting the Reindeer in Southern France," described at page 59, we have already alluded to.

Plate IV., "Hunters Feasting on Horseflesh in Southern France." Four trowsered, half-bare, tattoed men, sitting, kneeling, squatting, and crossed-legged, with a shaggy dead horse near by, and a roughly clad man coming along with something heavy on his shoulder. One of the feasters is talking and pointing excitedly, or toasting a bit on his spear (?). Two Indian wigwams are placed at the side.

Plate V., "Hunters in Rock-shelter at Night in Southern France," is reproduced here by permission. The interior of a cave, with skin (?) hangings at the doorway, into which a trowsered and otherwise clad man is coming. Two bare little children are near the fire on the floor, one lying down, and one standing up with a long marrow-bone in her hand. The parents are sitting on a ledge of the cave; the man nude, and either scraping or carving a bone (?), apparently with only one leg, but rapt in artistic ecstasy. The woman, also unclothed, but having an armlet, and apparently sewing a long-haired skin (?) with long sinew thread. It is probably after supper, as the cooking sticks for roasting (?) are not at the fire now.

Plate VI., "Swiss Lake-dwellers; an evening scene, Bronze age." The full-dressed man is paddling home in his dug-out; the children, thinly clad, look out from the railing; the woman, wearing a black dress with a tartan-plaid skirt, is fishing with rod and line and float; cattle are coming along the bridge, followed by two men carrying a dead deer.

Plate VII., "An Interment in a Long Barrow, Later Stone Age." A dark group of mourners, bearing a corpse, which is tied up nose and knees (in orthodox fashion), towards the entrance of a chambered burial-mound.

Plate VIII., "The Warrior's Courtship, Denmark; all the clothing, weapons, ornaments, taken from actual discoveries in the peat of Denmark. Bronze Age." This rests upon Dr. Sophus Müller's outline restorations in his "Voroltid." The modest-looking young woman, without shoes and stockings, with her water-jar at hand, has been gently detained by a good-looking, trowserless spearman. Possibly he will abduct her (according to the custom of marriage by capture) if she will not go with him willingly.

Plate IX., "Two British Warriors. Bronze Age." Specimens of cavalry and infantry, better than the Roman writers describe them. It is pleasant to see the old Britishers rehabilitated.

Plate X., "The Construction of Stonehenge" by the Dwarfs (!) according to H. N. Hutchinson, C. H. Read, and Flinders Petrie, see page 279.

AMERICAN ETHNOLOGY.

THE THIRTEENTH ANNUAL REPORT OF THE BUREAU OF ETHNOLOGY. By J. W Powell. Pp. lix., 462, pls. lx. Washington: Smithsonian Inst., 1896.

THE Annual Reports of the Bureau of Ethnology, addressed to the Smithsonian Institution, have so long been amongst the most valued publications devoted to the study of human races, both living and extinct, that general laudatory remarks are unnecessary on the appearance of a new volume. Suffice it to say that the consistently high standard of excellence observable in the previous volumes of

HARVARD COLLEGE LIBRARY

HUNTERS IN ROCK-SHELTER AT NIGHT IN SOUTHERN FRANCE.

(From a drawing by Cecil Aldin, illustrating H. N. Hutchinson's "Prehistoric Man and Beast.")

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To face p. 54.]

the series is fully maintained in the report issued in 1896, under the able directorship of Major J. W. Powell.

In his general introductory remarks, the Director gives information regarding the organisation and operations of the Bureau. He describes the gradual extension of the lines of research promoted by it, as new fields of investigation suggested themselves as necessary, in view of a complete elucidation of North American ethnology and archæology. Incidentally one may gather some idea of the enormously rich and seemingly inexhaustible field of exploration open to American investigators within the confines of their own national boundaries. One cannot but be struck with the great activity of researchers in ethnology in the United States, under the auspices of a government which liberally contributes funds for that most important branch of study, and lament the scarcity of funds devoted to the purpose in our own country.

The first paper in the volume is one by Professor W. H. Holmes on "Prehistoric Textile Art of the Eastern United States." This writer has already devoted much attention to the subject, and his skilful restoration models of ancient textiles from their negative impressions in clay (pottery, etc.) are well known. The work is important as forming a portion of the investigations of the condition of the Mound-Builders, conducted with a view to determining who these people were, and what relationship they bear to the modern Indian. Professor Holmes' researches have borne out those of others in other branches of the subject, and tend to identify the culture of the Mound-Builders with that of the modern Indians, who are presumably their descendants. Perishable structures, such as textiles, are apt to be conspicuous by their absence amongst prehistoric remains; but, as Professor Holmes relates, he has drawn his material largely from fragments preserved accidentally by contact with copper, whose oxides have proved preservative, or through the agency of salts contained in the earth, or by charring, or, as already stated, from negative impressions of textiles in burnt clay. The many illustrations are clear and well chosen.

A paper upon "Stone Art," by Mr. Gerard Fowke, surveys the different articles of stone found in the ancient mounds, and under various circumstances, on or below the surface of the ground, together with those obtained from living Indians. Here again, a comparison of the stone art-products of the Mound-Builders with those of the modern Indian, reveals a strong kinship between the two, helping to link their ancient and recent makers into one family. The classification adopted in the paper is one which has its conveniences, especially for cataloguing purposes, but its value is practical rather than scientific. Classification is difficult where function is unknown, and such group names as "Banner-stones," "Gorgets," "Cones," etc., must of necessity remain indefinite and unsatisfactory until the use of the articles comprised within the groups has been determined. The tables of geographical distribution are excellent for reference, and the illustrations numerous and good.

Elaborate surveys of aboriginal remains in the Verde Valley, Arizona, are described by Mr. Cosmos Mindeleff, who gives a sketch of the physical features of the district, which have played so important a part in determining, here as elsewhere, the nature of the habitations and mode of life of the inhabitants. The remains of stone-built villages, isolated lodges, and the curious and interesting cave-dwellings, are described in detail; and extensive irrigation works proclaim ingenuity in agricultural processes and skill in overcoming a natural

aridity of soil. The "cavate" lodges were evidently applied largely to purposes of defence. Mr. Mindeleff refers the occupancy of this region, as indicated by the ruins, to a comparatively late period. Many excellent photographs and sketches are reproduced.

The Rev. J. O. Dorsey supplies a study of the Omaha tribe, or rather a description of their dwellings and implements, the result of personal observations dating from 1878. The Omahas were a truly warlike and hunting people, migratory in their habits, a characteristic which is at once revealed by the merest glance at their various appliances. For them, as for so many North American tribes, the extinction of the bison has led perforce to a gradual change in habits. The paper contains much information in a condensed form, and the descriptions notice details too often omitted in ethnographical accounts.

A second paper by Mr. C. Mindeleff is devoted to a description of the important "Casa Grande Ruin" of Arizona, discovered in 1694 by Padre Kino, and in ruins even then. It is, perhaps, the best known example of aboriginal architecture on a large scale in the United States, and is now being carefully preserved with its walls strengthened and supported, a special grant of money having been voted by the Senate for that purpose.

The last paper is one on "Outlines of Zuñi Creation-Myths" by Mr. Frank Hamilton Cushing, who is, perhaps, the chief authority upon matters connected with the inner life and philosophy of the natives of New Mexico. Few modern primitive peoples have received so much and such careful study as have the inhabitants of this region, and Mr. Cushing's initiation into a tribe, even into its "priesthood," has given him unrivalled opportunities for the investigation of the doctrines and folk-lore of the people. The importance of a study of myths and legends rests largely upon the light, shadowy it is true, which is thrown upon the early history and migrations of a tribe; and the cosmogony of the Zuñis is interesting, not only *per se*, but also because of the side-lights which illumine the often ill-defined transitional periods of culture. The creation-myths of the Zuñis are, in fact, highly suggestive, and have a value apart from their mere intrinsic interest. Mr. Cushing's style of writing is admirably adapted to the description and translation of the legends and folk-tales of the Zuñi Indians. Throughout them runs a truly poetic strain, and the primitive, though often somewhat intricate, philosophy is tempered with many a graceful conception and poetical allusion, whose quality can only be rendered by a sympathetic writer. The paper is but a portion of a wider survey of the subject, and its author promises fresh communications upon the cultural characteristics of the Zuñis, with a view to elucidating further the meaning of the Myths of Creation.

H. BALFOUR.

BRITISH ARCHÆOLOGY.

THE RELIQUARY AND ILLUSTRATED ARCHÆOLOGIST. Edited by J. Romilly Allen. New series, vol. ii. 4to. 260 pp. Plates and many illustrations. London: Bemrose & Sons, 1896. Price 12s. nett.

THIS exceedingly interesting and well-illustrated quarterly, which has now reached the second volume of a new series, is devoted to the study of the early pagan and christian antiquities of Great Britain; mediæval architecture and ecclesiology; the development of the arts and industries of man in the past ages; and the survivals of ancient usages and appliances in the present.

As might be expected, we have a valuable paper on the Cup-

and-Ring sculptures by the editor, those of Ilkley-in-Wharfedale, the very interesting curved swastika of Woodhouse Crag, and a picture of the Doubler Stones on Silsden Moor, being reproduced among the many illustrations. Mr. Alfred W. Johnston writes on the "Dwarfie Stone" of Hoy, Orkney. This is a block of Old Red Sandstone, 28 feet, by 14 feet, by 3 to 6 feet high, and has been excavated near the larger end, to form two chambers, a large stone lying alongside having apparently served at one time as a door. The various legends connected with the stone, which was first referred to in 1529, are discussed, but Mr. Johnston inclines to the belief that it was originally a place of sepulture. Other papers in the volume, of special interest to us, deal with flint implements, and an exploration of Rains' Cave in Derbyshire, a note on which will be given in our Notes and Comments.

More Reliquiæ Diluvianæ.

The Testimony of Science to the Deluge. By W. B. Galloway, M.A., Chaplain to the Earl de Montalt. 8vo. Pp. vii., 166. London: Sampson Low, Marston & Co. [no date: 1896?]. Price 6s.

The object of this book is to prove that the "so-called glacial deposits" are due to a great deluge produced by the shifting of the earth's axis of rotation, any sudden change in which would undoubtedly have interesting results. The author's main qualification for the difficult task of the reconciliation of the teachings of Moses and Lyell apparently is that he is chaplain to the Earl de Montalt. The author is conscious of the importance of his mission and has been at elaborate pains, he tells us, "to make double sure of every step of" certain mental processes which he fondly calls "his reasoning." That we should not dignify them with this name is probably due to deficiencies in the author's expression rather than in anything else. He tells us "of a Great Ice Age of enormous extent, depth, and duration." We should not be surprised to hear that the author regards an ice age as the same thing as an ice-sheet; but if he does not, perhaps in his next edition he will be good enough to explain what is the extent and depth of an age. By the science of to-day the author sets little score; but there are hopes for it in the future, for he assures us that "science will right itself." He seems, however, doubtful about it, and so does his best to help the somersault. There is very little in the book that we agree with, so we hasten to quote his maxim that "science does not rest on the authority of names." Assuredly it does not rest on the authority of the names he adduces in support of his propositions; for he goes to Cuvier and Josephus for anatomy, to "a man of science" (Mr. Mungo Ponton, F.R.S.E., to wit), for biblical criticism, to the late Dr. Pye-Smith for botany, to Job for terrestrial physics, and to Sir William Dawson and Professor Hull for geology. The author had hopes of Sir Joseph Prestwich, but he died still encrusted by a tenacious theory; so that another good man is lost. The most important information we have derived from a perusal of this work is that the Earl de Montalt keeps a chaplain.

Good News for South Africa.

Stanford's Compendium of Geography and Travel. (New Issue). **Africa, Vol. II. South Africa.** By A. H. Keane. Pp. xvi., 671, with numerous illustrations. London: Edward Stanford, 1895. Price 15s.

This volume represents an enlargement of half of Keith Johnston's once useful work on Africa, of which the last edition was issued in

1884. Information has been accumulating at such a pace and recorded in so many languages, that it was hardly to be expected that the work could be made as complete as the original edition was at the time of its publication. Mr. Keane tells us that "in some instances many carefully prepared pages have had to be greatly modified and even rewritten" during the preparation of this edition. It is no doubt due to the haste of these alterations, that numerous errors have crept into the work. The editor thinks the volume will be "welcome to students unable to consult the innumerable books of travel, scientific periodicals, and memoirs" on South Africa; but we hope that no serious student will trust to the present volume as a summary of the results of even the most important works on that district. Thus in spite of all the recent explorations of the Cameroons, the author concludes his short list of references with the journey of Lieutenant Morgen in 1890-91, (p. 4). The ethnographical sections of this volume and the maps by which it is accompanied are excellent; but the rest is less satisfactory. The author's political judgment may be estimated by his naive reference to the time "when the little gap between Tanganyika and Mfumbiro is obtained by agreement with Germany and the Congo State," (p 333); and his political knowledge by the assurance of "the supremacy of the English language as the almost exclusive instrument of education throughout the South African Republic." We wonder whether the people of Pretoria will be as pleased at this assertion of the unimportance of the Dutch language, as the inhabitants of Johannesburg are sure to be by this rude exposure of their most notorious grievance. The author's competence to handle zoological problems may be gauged by the statement that "the Cameroons do not come within the zone of the true tsetse fly, which is here represented by the *Glossina*, an apparently closely allied but harmless species." The compiler of a book on South Africa might have known that *Glossina* is not a species but a genus; that the fly thus named is not harmless but deadly; and that instead of being closely allied to the tsetse-fly, it is the tsetse-fly itself, the name of which is *Glossina morsitans*. The author's choice of authorities is not always happy; it is amusing to those who have followed on Bateman's track, and know the nickname by which he is called on the Congo, to find him ranked with a few men such as Grenfell, Johnston, and Arnot, as one of the "careful observers" who have studied the Congo natives.

As an example of the author's speculations on physical geography we may quote from p. 27; "Ascension lies right in the track of the south-east trades, under whose influence, perhaps increased by the occasional crash of huge icebergs from the Antarctic regions, the Atlantic billows, twenty to thirty feet high, break with fury against the windward coast." Ascension is situated only 9° from the Equator, and the Antarctic icebergs do not come within thousands of miles of it; their crashing can have no more effect on the surf of Ascension than on the ripples of the Hampstead ponds.

The book, however, ought to have a good sale among members of the Stock Exchange, for its remarks on the mineral resources of Bechuanaland and Rhodesia are most encouraging. The author quotes (pp. 391-392) a long paragraph from that optimistic reporter Mr. F. Mandy, to the effect that the northern slopes of Mashonaland "will eventually prove to be the alluvial goldfields of the world. The neighbourhood of the Amazoe and its tributary streams is a veritable El Dorado." This the editor states to be borne out by recent research, a remark which ought to be very encouraging to city financiers when coming from such a geographical authority as Mr. A. H. Keane. The

French in Madagascar seem also to have hit upon a rich thing in gold mines, if the quotation on p. 608 is to be relied on; and it comes from a work by Zélie Colville, who, from the extent to which Mr. Keane quotes from her, is apparently *the* authority on the geography of Madagascar.

The Cell-Nucleus in Plants.

Die Morphologie und Physiologie des pflanzlichen Zellkernes; eine Kritische Litteraturstudie. Von A. Zimmermann. 8vo. Pp. viii., 188, with 84 figs. in text. Jena: G. Fischer, 1896. Price 5 marks.

Many botanists will probably remember that some years ago there appeared in the *Beihefte zum Botanischen Centralblatt* a series of useful abstracts of current cell-literature, from the pen of Dr. Zimmermann. Since that time an enormous amount of new facts has been accumulating concerning the cell-nucleus, and the object of the book before us is to place its readers *au courant* with the present state of our knowledge respecting this cell-constituent, together with such other protoplasmic structures as may be directly associated with it.

There is no doubt whatever as to the usefulness of the work. The author has fulfilled the task he set before himself in a business-like and thoroughly conscientious manner, and he well deserves the thanks of all who wish to get an idea of what is doing in cytology. At the same time, though it is, perhaps, rather ungracious to say it, we cannot help finding the book rather dull. The reason lies in the fact that the author, in his anxiety to omit nothing which may be considered as in any degree important, has poured out torrents of details, and these lie thickly strewn over his pages, with little or nothing to indicate their co-ordinate or subordinate relationships one with another. It is all very well to present, in a concise form, the facts and the conclusions which their discoverers may have drawn from them; but if the reader is to get much of a living idea of what it is all about, he must have already devoted a good deal of time to the first-hand literature himself. In short, we confess the book strikes us as being rather too much of a note-book—a good note-book, it is true, tolerably complete, and containing very few mistakes withal, but a note-book still.

And seeing that Dr. Zimmermann has proved himself to be an able investigator in the domain of cytology, it is the more to be regretted that he has not seen fit to adopt the *rôle* of exponent a little more definitely. No doubt it is premature to expect to found theories which are likely to prove permanent, and possibly this consideration may have had due—we had almost said undue—weight with the author when he was outlining the plot of his book.

But, after all, our criticism is chiefly directed against the method which Dr. Zimmermann has decided to adopt; for the way in which he has followed it out we have little but praise. The clear and succinct paragraphs, in which he deals with the memoirs of the huge number of authors whose works he cites, will earn for him the gratitude of many who are debarred, by lack of time or opportunity, from consulting the originals.

The book is divided into two sections. The first, or general, part contains an account of: 1. Methods of fixation, &c.; 2. The chemical composition (so far as it can be regarded as possibly known, of the nucleus; 3. The phenomena of Karyokinesis; 4. The physiology of the nucleus. The second part discusses the nucleus as it occurs in the various groups of the vegetable kingdom, and the

volume concludes with a good bibliography and index of the whole subject.

Thus, as will be seen by this synopsis of its contents, the book is a thoroughly useful, and we may add a reliable, work of reference to the literature on the cell-nucleus in the vegetable kingdom.

J. B. F.

The Booth Birds.

Catalogue of the Cases of Birds in the Dyke Road Museum, Brighton. By E. T. Booth. Second edition (by A. F. Griffith). 8vo. pp. 217, with 16 full-page illustrations. Brighton: King, Thorne & Stace, 1896.

This is a Museum guide-book, not only admirable for the ordinary intelligent public, but containing much of special interest to the field-ornithologist. It is an illustrated reprint of the late Mr. Booth's "Catalogue" of his collection published twenty years ago, and now that this collection has become the property of the Brighton Corporation, the work appears as an official handbook. Apart from a brief history of the bequest there are very few additions to the letterpress, which is based almost entirely on Mr. Booth's personal observations; but the exquisite photographs of the cases with which it is illustrated add greatly to its value, while the portrait of the author and the view of his exhibition room are interesting. The observations on the birds are desultory and make no pretence to systematic treatment, but we think the utility of the volume would be much enhanced if the scientific name of each species were placed after the popular name which is always employed.

Baran.

In common with the *Times*, *Ally Sloper*, and the *Jam-i-jamshed* we have received from Balmokand, cloth-seller of Rawalpindi, a book published at Lahore, entitled "The Priceless Gem," pp. 242, price (including postage) one rupee, of Mr. Balmokand. "The Priceless Gem" sets forth the advantage and necessity of "Baran," the disregard of which in India's degenerate days has "vitiated its [India's] original form and has occasioned the disappearance of those Excellencies (*sic*) and the prevalence of these evils and vices."

"Baran" seems to signify the Eastern equivalent of the Western Temperament, and the four different Barans are set forth at length on pp. 36–38. The work attempts to prove the advantages of marriages between those of similar Baran. Mr. Balmokand is evidently a great reader both of his own and of English literature; and the volume will no doubt be of considerable interest to those who are interested in Indian customs or who make a special study of temperaments. It is translated from Urdu by J. W. Rockwell.

Varia.

L'Annuaire Géologique Universel for 1895 has made its appearance, and this time is confined to the geology of France and her colonies in Northern Africa. It is a great matter for regret that a universal record could not be kept up, for the old volumes were most valuable, but we still have cause to be thankful for the energy of Dr. Carez and his publisher, which gives us so useful a record of the geology of France.

The Winter Number of *The Artist* has been dedicated to a consideration of the art of Frederick Sandys. The public has now an opportunity of seeing the work of this exquisite draughtsman

whose art has hitherto been appreciated by a select few. We would especially draw the attention of botanists to the wonderful drawings of trees and tangled vegetation made at Fairlight, near Hastings, and the flowers and plants that Sandys has introduced into his classical subjects. The grace and tenderness of his chalk drawings of women, and his marvellous rendering of a woman's hair make one more than ever lament the praise that has been wasted on the decadents. Messrs. Constable & Co. are to be congratulated on this beautiful number of their magazine.

The *Westminster Review* for October contains an able article by F. M. Lyte, explaining the metric system and advocating its use.

The *Revue Internationale des Archives des Bibliothèques et des Musées* ceases with the publication of its ninth number (December, 1896). The three numbers devoted to museums form a volume of 160 pp., sold separately for 7 frs. 50, by H. Welter, Paris.

The *Journal of Physical Chemistry*, edited by Profs. W. D. Bancroft and J. E. Trevor, is published as a new monthly at Cornell University.

A new semi-monthly, entitled *Chemische Rundschau*, appeared on October 1st, edited by Dr. F. Peters, and published at Berlin. The annual subscription is 16 marks.

A new international bi-monthly, devoted to medicine and medical geography, is published at Amsterdam. Its editor is H. Peypers, and its title *Janus*. Papers are published in various languages, and each number forms a quarto of about 100 pages.

Further Literature Received.

Cambridge Natural History, vol. ii., A. E. Shipley and S. F. Harmer: Macmillan. Habit and Instinct, C. L. Morgan: Arnold. Marketable Marine Fishes of the British Isles, J. T. Cunningham: Macmillan. Present Evolution of Man, G. A. Reid: Chapman and Hall. Report of the Horn Scientific Expedition, pts. i. and iv.: Dulau. Catalogue des Bibliographies Géologiques, E. de Margerie: Paris, Gauthier-Villars. The Cell in Development and Inheritance, E. B. Wilson: N.Y., Macmillan. Treatise on Deep-Sea and Pelagic Fishes, &c., G. B. Goode and T. H. Bean: Smithsonian Inst. British Lepidoptera, vol. iii., C. G. Barrett: Reeve. Determinative Mineralogy, G. J. Brush, revised S. L. Penfield: New York, Wiley.

Physical and Social Heredity, J. M. Baldwin: *Amer. Nat.* Early History of the Manchester Geol. Soc., M. Stirrup: *Trans. M.G.S.* System der Nordamerican. Schmetterlinge, A. R. Grote: *Mitt. Roemer Mus.* On the Fate of the Blastopore, K. Mitsukuri: *Journ. Coll. Sci. Tokyo.*

Nature, November 26, December 3, 10: Literary Digest, November 14, 21, 28, December 5; Revue Scientifique, November 21, 28, Dec. 5; Irish Nat., December; Feuille des Jeunes Nat., December; Amer. Journ. Sci., December; Naturæ Novitates, October (20), November (21); Science, November 13, 20, 27, December 4; Scott. Geogr. Mag., December; Westminster Rev., December; Botanical Gazette, November; Review of Reviews, December; Pop. Sci. News, December; Knowledge, December; Photogram, December; Journ. Essex Tech. Lab., October. Journ. Marine Zool., Sept. Bol. Mus. Para, Oct.

OBITUARY.

SIR BENJAMIN WARD RICHARDSON.

Born October 31, 1828. Died November 21, 1896.

IN Sir Benjamin Richardson science has lost a busy worker in many fields. Essentially a physician, he was at the same time an active experimentalist in pharmacology, and a prominent worker in the field of hygiene. To these labours he added those of a prolific writer on many topics, and of an ardent temperance reformer. In each of these fields he accomplished much valuable and enduring work, and if in none of them he reached the foremost rank, this is almost a necessary corollary to the many-sidedness of his character.

He was destined from boyhood for the medical profession, so that his aptitude for physical and natural science was early encouraged. At seventeen he was apprenticed to a country surgeon named Hudson, whose tastes lay in the direction of electrical science, and he proceeded thence to Anderson's College, Glasgow. In 1847 he returned, for the benefit of his health, to England, and became assistant first to Mr. Brown, of Saffron Walden, and later to Dr. Robert Willis, of Barnes, who was at that time editor of the *Medical Gazette*. Here he first turned his hand to literary work and became a frequent contributor to the *Gazette* and other periodicals.

He qualified at Glasgow in 1850, and in 1854 took the degrees of M.A. and M.D. at St. Andrew's. In 1854 also he won the Fothergill Medal of the Medical Society for a thesis on "Diseases of the Fœtus in Utero." He was now appointed lecturer on Medical Jurisprudence, and, in 1857, lecturer on Physiology to the School of Medicine in Grosvenor Place, holding the latter post until 1865, when the School was incorporated with St. George's Hospital. During this period, he became intimate with Douglas Jerrold, Thackeray, George Cruickshank, and other literary celebrities of the time. He became a Member of the Royal College of Physicians, London, and in 1865 was elected a Fellow of that body: two years later he became a Fellow of the Royal Society.

Space fails us to record all the investigations which Richardson undertook during his busy career, but among the more important were his researches into the subject of anæsthetics and the question of alcohol. He experimented largely on a variety of bodies to test their anæsthetic power, and to him is due the introduction of methylene bichloride as an anæsthetic, and the local application of ether spray for producing insensibility in minor operations. His

researches on the action of amyl nitrite were likewise of importance. From a humanitarian point of view the most striking outcome of his experiments has been the 'lethal chamber' now employed at Battersea for the painless destruction of superfluous dogs and other domestic animals. As the result of his investigations on the action of alcohol on human beings, he became a total abstainer, and in 1874 delivered the Cantor lectures at the Society of Arts on the subject of alcohol. He was Physician to the Temperance Hospital, and his influence in the promotion of temperance was wide and far-reaching, based, as it was, on sincere conviction and actual experiment.

He was deeply interested in questions of public health, and in 1862 he started the *Journal of Public Health and Sanitary Review*, and edited it for four years: to this he contributed some valuable papers, especially one on the hygienic treatment of phthisis. Later he inaugurated the short-lived *Social Science Review*, to which he contributed several poems and plays. His most important literary effort was, however, *The Asclepiad*, a quarterly periodical which for twelve years he produced—single-handed. Few men of his profession have been more widely known to the public, but this arose less from his actual professional abilities, great though these were, than from the part he took in allied questions, and especially those of hygiene and temperance reform. He was knighted in 1893, mainly in recognition of his services to the humanitarian cause.

WE regret to record the following deaths:—On November 20, DAVID ROBERTSON, the Naturalist of Cumbrae, of whom a longer notice will appear in our next; ARTHUR DOWSETT, President of the Reading Natural History Society, and the owner of valuable ornithological and entomological collections, on November 6; Dr. G. W. CHILD, lecturer on botany in St. George's Hospital Medical School, on December 1; the mycologist and florist. TH. KING, in Fochabers, N.B., aged 62; W. F. AINSWORTH, explorer, well-known for his researches in Asia Minor; Dr. J. A. MOLONEY, African explorer, aged 38, at Surbiton; J. W. RETGERS, known for his work on Isomorphism, on August 9, aged 39; Dr. E. WENZEL, Associate Professor of Anatomy at Leipzig, on October 25, aged 56; Dr. FRITZ WESTHOFF, zoological assistant at the Kgl. Akademie, Münster; in Munich, J. FESER, Professor of Pharmacology in the Veterinary College, aged 56; Professor E. VON WOLFF, author of the celebrated work "Landwirtschaftliche Fütterungslehre," and for forty-two years Professor of Agricultural Chemistry at Hohenheim, aged 78; Dr. A. H. POST, author of "Grundriss der ethnologischen Jurisprudenz," at Bremen; Dr. E. BAUMANN, Professor of Physiological Chemistry in the University of Freiburg i. B., noted for his researches on metabolism and cystin, and the discoverer of iodine in the thyroid gland, on November 2, aged 49.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

THE following appointments are announced:—Lord Reay, to be President of University College, in the place of the late Sir John Erichsen; L. E. Shore, to be Lecturer in Physiology at Cambridge, in place of Dr. A. S. Lea, and A. Eichholz, to be additional Demonstrator in Physiology; Dr. Hugo de Vries, to be Director of the Botanic Garden in Amsterdam, in place of Dr. Oudemans; Dr. R. H. Saltet, to be Professor of Hygiene at Amsterdam; Dr. C. Julin, to be Professor of Anatomy at Liège; Dr. A. Möller, of Idstein, to be Lecturer on Botany in the Forestry Academy at Eberswald; Dr. J. Blaas, of Innsbruck University, to be Assistant Professor of Geology and Palæontology; Dr. Karl Möbius, Professor of Zoology in Berlin University, to succeed Professor Beyrich as Director of the Natural History Museum there, and Dr. W. Dames, to be Director of the Geological Department; Dr. H. Traube, to be Professor of Mineralogy, and Dr. O. Seeliger, to be Professor of Zoology, in Berlin University; Dr. K. Mez, to be Professor of Botany in Breslau University; Dr. Emil Schmidt, to be Professor of Anthropology in Leipzig University; Professor S. L. Schenck, to be Professor of Embryology at Vienna; Dr. T. Beer, to be Privatdocent in Comparative Anatomy at Vienna; Drs. B. Issatschenko and M. Grimm, of St. Petersburg University Botanical Laboratory, to be Assistants in the State Laboratory of Agricultural Bacteriology; Dr. Wladislaw Rothert, to be extraordinary Professor of Botany at Kazan University; J. S. Wright, to be Lecturer in Botany at Indiana Medical College; H. M. Richards, to be Lecturer of Botany at Barnard College, New York City; E. V. Wilcox, of Cambridge, Mass., to be Professor of Biology in Montana State College of Agriculture; Dr. Seitaro Goto, to be Professor of Botany in the First High School at Tokyo.

SIR JOSEPH LISTER and Professor Michael Foster have been elected honorary members of the Asiatic Society of Bengal, in place of Huxley and Pasteur.

Professor Albert Heim, geologist, of Zurich, has been elected a foreign member of the Royal Society.

Mr. Michel Levy has been elected a member of the Section of Mineralogy of the Paris Academy of Sciences, in succession to the late G. A. Daubrée.

Professor O. Brefeld, the eminent fungologist, of Münster, has been made Geheim-Regierungs-Rath.

DR. MÜLLER, Director of the Königsberg Zoological Garden, has retired. Dr. H. G. Hallier has resigned his position at the Buitenzorg Botanical Garden and is returning to Germany.

THE Hayden Memorial Prize for 1896 has been awarded by the Philadelphia Academy to Professor G. Capellini, of Bologna.

A STATUE of Charles Darwin, presented by Professor Poulton, is to be erected in the court of Oxford University Museum, which is also to be adorned by a bust of Sir Henry Acland.

A WING for scientific purposes is to be added to the buildings of Aberdeen University at a cost of about £10,000, which will be defrayed by the city in return for land given by the University Court.

A SCHOOL of science has been founded at Madrid, supported by the state. It comprises twenty-seven professorships, of which *L'Anthropologie* mentions the following, with the professors appointed to occupy them:—Physiological Psychology, Professor Simmaro; Origin of the Castilian Language, M. Pidal; Introduction to Sociology, Azcarate; Structure and Activity of the Nervous System, Ramon y Cajal; Anthropology of Spain, M. Anton; General Evolution of the organic and inorganic kingdoms, Cortarar.

A BACTERIOLOGICAL laboratory in connection with the veterinary department of Pennsylvania University has been established under the directorship of Dr. M. P. Ravenal to investigate the diseases of poultry and cattle.

THE visitors to the Huddersfield Economic Museum during the past year numbered 878, we learn from the report published in its organ, the *Naturalists' Journal*, for December. Mr. Mosley is a little disappointed with the lack of support, both to the Museum and the *Journal*, but is hopeful of the future. The Museum will not be open on Sundays, but visitors can be admitted if notice be given beforehand.

WE learn from the *Halifax Naturalist* that the Museum of Halifax (Yorkshire) is being removed to new quarters at Belle Vue. The borough of Todmorden has received from the local Botanical Society the gift of its library, and botanical, geological, and mineralogical collections.

AN ethnographical museum is to be founded at Budapest, of which the objects exhibited in the ethnographical section of the Millennial Exhibition are to form the nucleus.

DR. L. SERRURIER, Director of the Ethnographic Museum at Leyden, has resigned, because the state refuses to rebuild the museum.

THE balance over from the local fund, after paying the expenses of the British Association meeting this year, is about £980, which is to be invested, and the interest devoted to furthering the scientific work of the Liverpool Marine Biology Committee.

THE following are among the forthcoming afternoon lectures at the Royal Institution:—On Tuesdays, from January 19 to April 6, Professor A. D. Waller, on Animal Electricity; Professor H. A. Miers, three lectures on Some Secrets of Crystals, on January 21, 28, and February 4; Dr. J. W. Gregory, three lectures on the Problems of Arctic Geology, February 11, 18, 25; Professor W. Boyd Dawkins, three lectures on the Relation of Geology to History, on March 25, April 1, 8. The Friday evening meetings, for members and friends only, include Professor J. Milne, on Recent Advances in Seismology, February 12; Lieut.-Col. C. R. Conder, on Palestine Exploration, February 26; S. Bidwell, on Some Curiosities of Vision, March 5; Sir W. Turner on Early Man in Scotland, March 26.

AT the request of Professor Meldola and Mr. T. V. Holmes, president and secretary, respectively, of the Corresponding Societies Committee of the British Association, Professor G. S. Boulger, on December 12, delivered an interesting and suggestive address, before the Essex Field Club, on the "Federation Ideal for Natural History Societies, with Special Reference to the Eastern Counties." He dwelt on the scientific importance of the organisation of all the natural history societies, and pointed out the benefits that would accrue from joint work, mutual help, and joint publication, and the need of more individual workers. Communica-

tions from various local societies of East Anglia were laid before the meeting, and a lively discussion followed. Those interested in the subject may be referred to papers entitled "What shall we do with our Local Societies?" by Professor G. S. Boulger, (NAT. SCI., September 1896), and "The Organisation of Local Science," by G. Abbott (NAT. SCI., October, 1896).

THE German Fisheries Association offers a prize of 600 marks for the best essay on the history and biology of *Leptomitus lacteus*, with special reference to its appearance and disappearance in impure water. Papers must be sent in before May 1 next, to Mr. Weigelt, 90–91 Zimmerstrasse, Berlin, S.W.

AN interesting lecture on "Flight, Natural and Artificial," was delivered by Dr. G. H. Bryan, F.R.S., in the Imperial Institute on November 30. The advantage of a screw propeller over a a flapping method was illustrated by some little models, which were thrown up, and floated in a very bird-like manner. Lantern slides of various old and new machines were exhibited, and Mr. P. S. Pilcher's machine, which was displayed in the hall, was explained by the inventor.

DR. JOHN MURRAY, of the "Challenger," was on November 30 presented by the contributors to the Report with a handsome morocco album, containing all their portraits. The dedication plate was designed by Mr. Walter Crane.

ALTHOUGH the Russian universities are still closed to women, a medical school is to be founded for them in St. Petersburg.

THE Röntgen rays applied to palæontology, in the shape of skiagrams of fossils embedded in Chalk from near Rheims, were recently exhibited to the Paris Biological Society by Mr. Lemoin. The disputants over *Archæopteryx*, to whom we suggested this method of settling their differences, have not yet communicated their results to us.

THE quadrigentenary of Vasco da Gama's discovery of the sea-route to India, which was to have been celebrated at Lisbon in 1897, has been postponed till May, 1898, that being the month on which the explorer reached Calcutta 400 years before.

THE eleventh German Geographical Congress will be held at Jena on April 21, 22 and 23, when the subjects discussed will include biological geography and polar investigations.

AN international congress of experimental and therapeutic hypnotism is to be held in Paris in 1890.

IN consequence of unforeseen delays, it has been found desirable to extend the time allowed to competitors for the Welby Prize (announced in our last number) till January 1st, 1898. Professor Emile Boirac, Paris, is to be the French member of the Committee of Award.

MEXICO now ranks among the progressive nations who have legalised the use of the metric system. The Liverpool Chamber of Commerce, too, has unanimously passed a resolution in favour of adopting this system, and urging the necessity of bringing in a Bill for that purpose.

WITH reference to Dr. Ohlin's account of the Swedish Expedition to Tierra del Fuego, which appeared in NATURAL SCIENCE for September last, it is of interest to note that the first results of the Hamburger Magalhaessischen Sammelreise have just appeared. Lieferung I. is published by the Natural History Museum of Hamburg, and contains a general account of the expedition, by Dr. Wilhelm Michaelsen, and a particular account of the Vermes by different authors.

PROFESSOR D. G. ELLIOT says, with regard to his expedition into Somaliland, that he has obtained a very extensive collection, chiefly of the large mammals, probably the most complete ever brought out of any country by one party. No fewer than fifty-eight cases and barrels were shipped direct from Aden. After this we need not remind our readers that the destination of the barrels was Chicago. He also obtained over 300 specimens of birds, fish, insects, and reptiles: this is not much.

Mr. Bastard, who is exploring in Africa, has been prevented by the trouble in Madagascar from penetrating into the interior of that island. He has, however, made good collections of fossils, also anthropological measurements and photographs.

Mr. Voillot has returned from a voyage to Haute Mamberé; he brings with him ten Baya skulls and an interesting ethnographic collection. Another valuable anthropological collection is that brought back from Russian Asia by Mr. E. Blanc.

Of Mr. Alexander Whyte's explorations in the Karonga Mountains in Central Africa, we learn that the results include 6,000 dried specimens of plants, 5,000 land-shells, 3,000 insects, numerous mammals, reptiles, geological collections, and so on. The climate was far from healthy, and several members of the party suffered from cold and sickness.

The *Daily Chronicle* of December 5, gives some interesting news of the French Hourst Expedition to the country behind the Niger Coast Protectorate; from it we learn that, in spite of difficulties, the results have been very valuable, and that the whole party returned well after an absence of three years. The war-songs of most of the tribes have been taken down by the phonograph, silhouettes of native heads and many photographs were made; and the geological and geographical surveys are also very complete.

THE proposed Belgian Antarctic expedition, headed by A. de Gerlache, is to start on board the "Belgica," in the middle of next July. A laboratory is being constructed on deck for the scientific members of the expedition, who include a geological chemist, Mr. Archowsky; a meteorologist, Mr. Danco; a doctor and naturalist, Mr. Taguin; and Mr. Racovitza, who will conduct the dredging operations. The work of the expedition will be carried on so far as possible on the same lines as that of the "Challenger" expedition.

PROFESSOR PENZIG, of Genoa, editor of *Malpighia*, has undertaken a botanical expedition to Buitenzorg, Singapore, and Ceylon.

DR. GRÜNLING, Curator of the Mineralogical Collection in Munich, has gone to Ceylon on an exploring expedition.

AN agricultural experiment station, which will undertake very useful work, has been founded at Usambara, in German East Africa.

ON Thursday, December 17, almost exactly at 5.30 a.m., an earthquake shock, severer than has been felt for many years, affected almost the whole of England and Wales. The exact position of the seismic focus has not yet been determined, but Hereford seems to have suffered most, for here a woman died of fright, the pinnacles of St. Nicholas' Church fell, and a pinnacle of the cathedral was damaged. Premonitory shocks were felt at various places from about 3 a.m., and a few after-shocks were also experienced.

CORRESPONDENCE.

Geologists and Specialists.

It is not surprising that some geologists should have protested against your note on "Pavingstone Palæontology," because its language was not very happy and did not seem calculated to smooth the way for bringing geologists and zoologists into closer touch with one another, which it now seems was your real intention.

It is satisfactory, however, to find that you are able to respond to the letter of "Stratigraphical Geologist" by giving a preliminary list of specialists who are willing, under certain conditions, to examine and name fossils collected by field geologists. I have long felt that there ought to be closer scientific relations between the specialist in the museum and the geological worker in the field. The specialisation of our science has now reached a point when the one set of workers cannot achieve satisfactory results without the assistance of the other set. I heartily endorse the remarks made by Mr. Marr in that portion of his presidential address which you quoted in your November number (p. 285), and I would remind you that they included some good advice to the specialists, who must be careful not to take too comprehensive views of species, and must remember that they too have something to learn out of doors. As Mr. Marr says, "the specialist who remains in his museum examining the collections amassed by the labours of others, and never notes the mode of occurrence of fossils in the strata will perhaps soon be extinct." By all means let us join hands, and remember that we can each help the other, and that we each have much yet to learn from Nature.

Coming now to the question of publishing a list of specialists, I think you are quite right in maintaining the desirability of making it more widely known that there are so many specialists who are ready to help any *bonâ fide* worker by naming fossils for him. I have received much kind assistance from several of the gentlemen on your list, but there are others to whom I have not hitherto ventured to apply for assistance because I had no assurance that they would be willing to give it.

I am sure there are men belonging to natural history societies and field clubs in various parts of the country who are quite capable of working-out local geological details, but who are deterred from trying because they do not know how to get their fossils named; there are doubtless others who make the endeavour, and who publish lists of fossils which are not as accurate as they might be.

If, therefore, any of the palæontologists to whom you have written have withheld their names because they think it is unnecessary to give them, I can assure them from personal experience that they are mistaken.

I may likewise point out that there is also such a man as a stratigraphical specialist, a student of one particular set of rocks, and that he may sometimes act as a useful intermediary. For instance, I am quite willing to examine collections made from any Upper Cretaceous strata, provided localities and horizons are indicated, and to name the fossils to the best of my ability. Any species which I could not determine with certainty might then be forwarded to specialists either by me or by the collector himself.

I trust you will shortly be able to publish a longer list of names, and I shall hope to avail myself of such assistance still more fully in the future than I have done in the past.

A. J. Jukes-Browne.

[We beg to thank the six friends who have expressed their willingness to have their names added to our list of specialists. We intend to publish a supplement to the list next month.—Ed. Nat. Sci.]

ORANG-OUTANG OR ORANG-OUTAN.

DR. ARTHUR KEITH remarks in his valuable paper on Anthropoid Apes, vol. ix., p. 316, note, "Temminck gives Orang-Outan as the correct spelling; Sal. Müller, who was familiar with the Malay language, rendered it Orang-Oetan, but Orang-Outang is the form in most common use." If Orang-Outang really is the form still in most common use, it only proves how difficult it is to remove an inaccuracy which has once crept into literature. *Orang utan* means "man of the forest," *Orang utang* would mean "a man who has debts." As far back as 1877 (*Mitth. Zool. Mus.*, II., p. 225, note) I showed this to be the case, and, more recently (*Abh. Mus. Dresden*, 1894–5, No. 14, p. 6, note) I recurred to the question with the following words, which I beg permission to quote translated: "With what difficulty even small faults get out of books, is proved by the spelling of the word Orang utan. Mostly one still finds Orang utang, though Müller and Schlegel so early as the year 1840 drew attention to the falseness of this spelling (*Verh. Nat. gesch. overz. bez.*, p. 11); later, in 1866, Gratiolet and Alix censured it (*N. Arch. Mus. Paris*, II., p. 4, note 1). In 1877 I remarked thereon (*l.c.*), and recently Jentink recurred to the incorrect 'g' (*Notes Leyden Mus.*, 1895, p. 17, note)." "Oe" in outan is the Dutch spelling; "ou," the English and French; and "u," the German, according to the pronunciation of the respective languages.

R. Zoological Museum, Dresden, 9 *Dec.*, 1896. A. B. MEYER.

AUSTRALIAN PICTOGRAPHS.

WITH reference to our Note on this subject (vol. ix., p. 80), in which we called attention to discrepancies between the descriptions and the drawings published by Mr. R. L. Jack, that gentleman informs us that his descriptions are accurate, but that his faithful copies of the drawings were lithographed by a careless draughtsman, whose errors Mr. Jack had no opportunity of rectifying.

DR. WHEELTON HIND'S "MONOGRAPH ON *Carbonicola, etc.*"

IT is with repugnance and regret that we publish the following letters from Dr. Wheelton Hind and Mr. W. F. Holroyd. We have urged both of these writers to expunge the personalities that they have permitted themselves to substitute for argument. Since, however, both of them seem unable to appreciate our objection, and insist on the publication of this or nothing, we have finally decided to follow our practice of allowing free voice to either side. Therefore, after due warning, the letters are printed *word for word* as their authors finally sent them to us.

We could not ask our Reviewer to reply to letters of this nature in other terms than those chosen by him; but in defence of ourselves against Dr. Hind's fina paragraph, we may point to his own assertion that Mr. Bolton informed him last summer that he thought of writing a review of this Monograph. For the rest, we leave it to any of our readers that have a liking for this kind of thing, to contrast the statements now made by Messrs. Hind and Holroyd with the Monograph itself, and with the remarks of our Reviewer.—ED. NAT. SCI.

IN your number of Natural Science for Dec. 1896 there appears a review of my Monograph on Carbonicola, Anthracomya and Naiadites, the nature of which from more points than one makes it necessary for me to make some remarks thereon; not I hope that I for one moment object to any fair and honest criticism, which the work doubtless calls for, being the production of a mortal.

The reviewer objects to the substitution of the generic term *Carbonicola* M'Coy, for *Anthracosia* King, a replacement which I deeply regretted, but according to the present rules of nomenclature I had no choice in the matter. Unfortunately in endeavouring to show grounds for the retention of *Anthracosia* the reviewer suppresses one portion of the evidence, and invents another. Briefly the position is as follows.

Sowerby, in 1829, describes Min. Conch. 2 coal measure shells as *Unio acutus*, and

U. subconstricta. J. de C. Sowerby in 1840 described *U. robusta* Geol. Coalbrookdale. In 1844, Prof. King proposed the name "*Anthracosia* for a group of Unionidæ characteristic of the coal measures" without figures or description as the reviewer says a preliminary notice of the worst sort. In Dec. 1855 M'Coy published the 3rd fasciculus of his work on the Synopsis of the Classification of British palæozoic rocks with a Diagnosis of *Carbonicola* to which genus he referred the U. acutus, U. subconstricta, and U. robusta, of the Sowerbys and U. turgeda, Brown. A mistake was made in describing the genus as possessing a posterior lateral tooth. King on receiving a copy of this work immediately published in Jany 1855 (Ann. Mag. Nat Hist 1856) a description of *Anthracosia* and in a postscript dated Dec. 27, 1855, says, "My friend evidently thinks it synonymous with Anthracosia which he admits being aware I intended describing; However, if the genus *Carbonicola* possess the characters diagnosed by Prof. M'Coy, it is clearly not the same as my *Anthracosia* which does not possess lateral teeth." There can thus be no doubt as to the priority of Carbonicola for one mistaken character in a description cannot invalidate, but the reviewer drawing on his own fertile imagination says "M'Coy failed to understand the genus he described and his diagnosis could only have been drawn up from specimens of other genera from a younger formation." His accusation is completely wrong for M'Coy correctly referred the four species mentioned above quite correctly to his genus, where as King described a new specific type and only referred one of Brown's many forms to his genus. It turns out too that the form of hinge described by King obtains only in the species of C. aquilina, another of J. de C. Sowerby's types. I did suggest that the lateral tooth was inferred from the close affinity Carbonicola has to Unio, but there is not the slightest foundation for the reviewer's statement; as it will appear later, the reviewer knew of this change and the reasons for it long before the work was in type and he wrote me that though he regretted the change, it seemed to be enevitable.

My reviewer seems very troubled that species and varieties are not defined with mathematical accuracy. This is of course the resultant of his long application to museum work, but he would go further for in a letter dated Nov. 20, 96, he writes me "Your definition of species is very faulty but for this I blame the Palæontographical. They should establish a set of rules for the guidance of workers so that all who consult their publications may know what view is taken of specific distinction as a whole. As it is each man is a law unto himself and the modern palæontologist stands on one side and scoffs." It is what I imagine even you, Sir, would call "high falutin," but it argues the possession of a mind more adapted to mathematical formulæ than to Biology. On one hand, he demands mathematically defined species and on the other, grumbles that the way species seem to pass into each other is not better shown.

Another point objected to is the invention of species on one or two examples, and I cordially agree with him, but this is a matter of personal observation and it is the easiest and cheapest form of criticism to judge work in the absence of specimens. It is at times necessary to describe unique examples, but it should be done with great caution. In two of the cases the reviewer will be glad to learn that other specimens have turned up which I think justify my views. In the case of the figures of Anthracomya dolobrata the reviewer objects that the type differs from the other shells referred to the species. Here he is not making allowance for the imperfections of the type and consequently his arguments are valueless. He also suggests that C. subconstricta is the internal cast of C. robusta, but he has not noticed that Sowerby's type is a testiferous example. Several of the other figures of this species are those from which M'Coy redescribed this species and are in the Woodwardian collection Cambridge. One more example of the reviewer's methods will suffice. He says "the specimens 6 & 7 Pl ix. are taken as types of C. aquilina, but they might equally serve as elongated forms of C. acuta." It is evident to me that he has not yet arrived at the fundamental differences between my descriptions of these two species, but it so happens that the two shells in question were brought to my house by the reviewer himself and that it was largely on his suggestion that they were referred to C. aquilina. Reviewers should really be careful in little transactions of this kind or the value of their remarks is liable to be heavily discounted.

The reviewer finds it difficult to appreciate the differences between Anthracomya and Naiadites, and grumbles that I do not contrast them. I really did not think it necessary to contrast genera of two different Families. He quotes a remark I make about the difficulty of distinguishing compressed forms of one species when the periostracum only is left and would make out that I apply it to the whole genus. Finally the reviewer remarks that the shells "are not described with uniformity and that it is therefore difficult to compare a couple of descriptions." Surely this is pure imagination on his part, for on rereading them I can only find one example of a fault of this description i.e. in the case of C. Polmontensis.

After inveighing in several places upon the absence of indications of varietal forms, and the passage of species towards each other, he finds fault with the excessive number of figures, which are numerous simply to demonstrate the facts which the reviewer supposes absent. He grumbles that I have in any way allowed horizonal distribution to come into the determination of Species but the answer is that in the same bed the intermediate forms if any are always present but in beds separated by many feet of strata this sort of evidence is necessarily absent.

On p. 36, I find that I specially thank Mr. H. Bolton for "looking over my specimens, MS. and proof; and for several observations which have been of great service to me in the preparation of the section on *Carbonicola*." Surely the grave faults which he now finds in my work should, in common courtesy, have been pointed out before publication, in the present case he forgets that every criticism he makes is an attack either on his own accuracy of observation or on his character as a proof-reader and adviser. Is there no etiquette in a matter of this sort, or is it permissible to pretend to afford every help possible in the production of a work, but at the same time to make reservations on which an attempt may be made to pull the whole work to pieces at some future date?

Stoke-on-Trent, Dec. 3, 96. WHEELTON HIND.

YOUR ast issue contains a review by "H.B." of Dr. Hind's recently published Monograph of Carbonicola, Anthracomya and Naiadites." Your critic is evidently very inconsistent inasmuch as he points out, in the same sentence "signs of great haste" and yet considers the monograph as a "valuable contribution." If there is one thing more noticeable than another, in reading over the "Monog" it is that the author has been most careful and deliberate in dealing with what everyone who knows anything of the subject, considers a most difficult group to work. Evidence of this is given in the careful and elaborate study and delineation of the interiors.

Let us examine some of the so called omissions and commissions alleged by the reviewer, and first, the replacement of name "Anthracosia" by that of "Carbonicola." Dr. Hind's reasons page 39, vol. xlviii., are quite sufficient and conclusive to authorize him to drop "Anthracosia" and retain "Carbonicola." "McCoy" did describe the genus, even if he gave a rather erroneous diagnosis. "King" neither described nor figured. When, a year later, "King" described his Anthracosia, he gave the hinge and teeth of C. aquilina, a specimen which is not characteristic of a genus but only of a species.

The demands by H. B. for absolute fixity of a species and variety are absurd, inasmuch as the Carbonicola are so variable, and therefore we agree with Dr. Hind that it is better to take as types those specimens which are most typical, in a certain set from the same bed. It is further charged by "H. B." that no gradation of species is sufficiently recognized, but if so, why has Dr. Hind given such numerous figures —thirty-six for example of C. aquilina, Again, why does "H. B." think there are too many figures when he so clearly recognises the necessity of comparing a large series of specimens.

Another charge is that Dr. Hind fails to distinguish what constitutes a species. Will "H. B." kindly define what he thinks, exactly constitutes a species.? With regard to A. dolobrata and A. Adamsii, I have seen both specimens myself and consider Dr. Hind quite correct in his naming of them. Again—species are said to have been founded upon one or two specimens. This is true but, the said species have received due consideration, and such naming is sometimes quite unavoidable,

besides which, the matter has been fully justified in two cases by the finding since of other specimens. Further justification for naming single specimens is afforded, when they are so clearly differentiated from well known types, as in A. Lanceolata, page 105, vol. xlix. With regard to C. Rugosa and C. robusta, even if they should ultimately be proved to be of the same species, it would be obviously unfair to delete the species in the absence of perfect specimens.

As "H. B." ought to know, there is great difference between Anthracomya and Naiadites, there being nothing in common between them in perfect examples. Dr. Hind is therefore misquoted when he is said "to admit a very close resemblance in both crushed and perfect examples." He was speaking of the similarity of crushed forms of a single species, viz., A. lœvis var. Scotica.

It is a mistake of H. B.'s to say the various items of description are not in same order, on the whole, the diagnosis of the species are made with extreme care, and in proper order. Lastly reference must be made to "H. B.'s" previous knowledge of all he has revised.

On page 36 *Pal. Soc: Monog.* vol. xlviii we find statement that he intended himself originally to work up portion of the subject, and had already in M.S. a few pages of his paper. It also seems that he looked over the specimens selected for figuring, the M.S.S, and "proofs," of Dr. Hinds' Monog. Why then does he speak as he does of C. acuta, C. robusta, C. aquilina, &c.? Why did he reserve his criticism until after publication. Was it chagrin at the successful outcome of another's work?

Had "H. B." been as critically acute as he fancies himself to be, and as honestly critical as he ought to be, he would have been able to see, instead of "signs of great haste" implying careless work, signs of great painstaking, of persistent and accurate investigation, of unwearied research, and of the consummate skill of the "true" specialist with reference to the study and description of these difficult groups of Pelecypods.

W. F. HOLROYD, F.G.S.

I HAVE been favoured with the sight of the above letters regarding my review of Dr. Hind's Monograph. The review was signed only by initials. From these to jump to conclusions as to the authorship, and to publish, without permission, portions of private correspondence and garbled versions of our conversation, as Dr. Hind has done, is a course of action which must excuse me from entering into any discussion with him. Mr. Holroyd's position in this matter is like his arguments—not very clear; but to you and your readers I have only to say that I am ready to stand by everything written in my review.

HERBERT BOLTON.

The Manchester Museum, Owens College.

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